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The American Historical Review



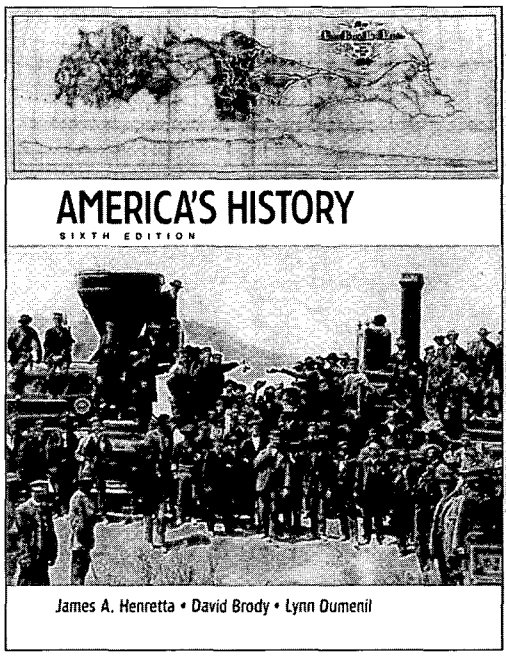
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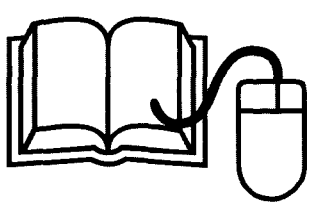
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In This Issue

The April issue contains two articles, a review essay, and an *AHR* Forum. The articles are both variations on a prevalent theme in our pages, transnational history with an imperial accent; the review essay surveys recent literature on European popular politics in the post–World War II era; and the Forum offers an exchange about a recent book dealing with the past and future of the practice of history. Six featured reviews are followed by our extensive book review section.

Articles

In "An Age of Imperial Revolutions," **Jeremy Adelman** explores the relationship between the crises of empires and the making of nations during the so-called age of revolutions in the late eighteenth and nineteenth centuries. Looking especially at the Iberian Atlantic, with comparisons to the British and French imperial spheres, he argues that colonies did not repudiate empire in the name of a new model of sovereignty, thereby simply hastening the demise of old imperial regimes. Empires had much more elasticity, and nations more contingent origins. Indeed, the article questions the familiar formulation of the age of revolutions as marking a definitive transition from empires to nations. Features of empires proved durable and found a place within successor nations. At the same time, the upheavals of the period revealed characteristics of sovereignty once present in imperial regimes that were not so easily reassembled under the mantle of nation-states.

"Reforestation, Landscape Conservation, and the Anxieties of Empire in French Colonial Algeria," by **Caroline Ford**, explores the ideological nature of forest conservation policies and their impact on the indigenous population in French colonial Algeria from the 1860s to the First World War. Ford notes that land management in European empires was hardly uniform; it depended upon local factors and particular forms of colonization. In Algeria, it was shaped more by patterns of settler colonialism, and by how the French conceived their country's historic role in North Africa, than by other factors, such as ecological conditions. Her article argues that France's policies toward the environment reflected deep-seated colonial anxieties about climate, race, and the relationship between "East" and "West" at a particular moment in the history of French colonialism. It also shows that French initiatives

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in this context reflected the intense imperial competition between European empires in the so-called "scramble for Africa" at the end of the nineteenth century.

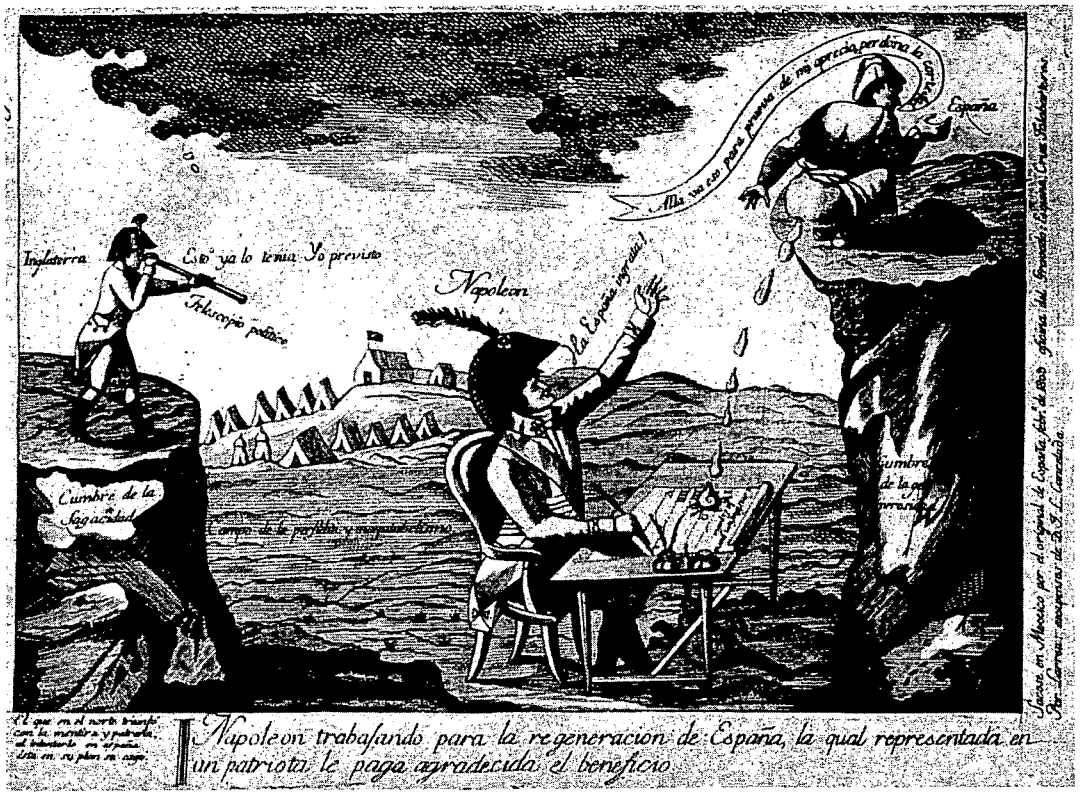
Review Essay

Recent histories of popular political activity in Europe offer new perspectives on postwar political history, the sources of "democratization," and political change more generally. In "What's Left? Popular Political Participation in Postwar Europe," **Belinda Davis** focuses on the importance of popular movements and their success in asserting the legitimacy of broad, usually extra-electoral forms of political involvement. By looking at unconventional sorts of activism, recent historians have illustrated the patterns of informal opposition to regimes and governments by a wide range of groups and populations and even across a divided continent. They also have suggested that regular and repeated mobilizations fostered the process of recasting and expanding democracy. Davis frames this work as a challenge to conventional political concepts, such as "revolution" and the ideological division between right and left. She also notes that many authors have been forced to struggle with this challenge; many too have confronted the difficulty of writing history as a historian-activist.

AHR Forum

The subject of the *AHR* Forum in this issue is Geoff Eley's book *A Crooked Line: From Cultural History to the History of Society* (2005), which provides at once an analysis of the various transformations of historical thinking in the last forty years, a personal narrative of how one historian experienced them, and an argument for how we should proceed in their wake. Three historians with different perspectives and expertise offer their comments on the book, followed by a response from the author himself. **William Sewell** takes issue with Eley's explanation for the transition from social to cultural history, pointing to larger forces at play relating to transformations in the nature of capitalism, forces that are not easily overcome. **Gabrielle M. Spiegel** suggests alternatives to Eley's arguments for ways to recapture some of the methodological virtues of social history while holding on to the insights of cultural history. Finally, **Manu Goswami** offers a critical though appreciative reading of Eley's book, with particular emphasis on the place of subaltern studies in recent historiography. The concluding essay by **Geoff Eley** is in part a restatement of some of the principal arguments of his book, and in part a response to these three comments.

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This image, originally made to circulate in Spain in February 1809, spread throughout the empire. It depicts Napoleon (seated in the middle) composing decrees and constitutions for the benefit of others, including occupied Spain. England (represented at left) gazes from "the summit of wisdom" through its "political telescope" at new opportunities. Meanwhile, Spain (crouched at upper right) expresses its own sentiments from "the summit of generosity," explaining, "Here is proof of my appreciation, excuse the politeness." "Ungrateful Spain!" replies Napoleon. The caption at the bottom reads: "Napoleon, working for the regeneration of Spain, represented as a patriot who is gratefully paying for his reward." Archivo General de Indias, Estampas, 5.1.

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An Age of Imperial Revolutions

JEREMY ADELMAN

When the Venezuelan creole Francisco de Miranda led an expeditionary force to the shores of his native land to liberate it from Spanish rule in the summer of 1806, he brought with him a new weapon for making revolutions: a printing press. He hoped that his band of white, black, and mulatto patriots would start a revolt to free a continent with an alliance of swords and ideas. After dawdling for ten days, Miranda learned that royal troops (also white, black, and mulatto) were marching from Caracas. He withdrew before the two multiracial forces could clash. Consider Miranda's reasons for retreat: The nation he sought to free from its chains was not, in his opinion, a nation at all. While Venezuelans yearned for "Civil Liberty," they did not know how to grasp and protect it. They needed a liberation that would tutor them in the ways of liberty and fraternity, to create a nation of virtuous citizens out of a colony of subjects. This was why Miranda treated the printing press, a portable factory of words about liberty and sovereignty, as part of the arsenal of change: he wanted to create public opinion where there was none. But faced with the prospect of a violent clash and a scourge of "opposition and internal divisions," of a war waged mainly with swords, he preferred to pull out and bide his time.[1]

Miranda's dilemma—whether or not to move forward knowing how revolutions worked in imperial settings when their protagonists did not presume that their cause was self-evidently bound to triumph—evokes questions about the embedded politics of what we might now call, with a wince, "regime change." As empires gave way to successor systems in their colonies, those regimes began to call themselves nations not in order to cause imperial crises, but as the result of such crises. The study of imperial crises and the study of the origins of nationalism in colonial societies should inform each other more than they do. Bringing these two separate fields of scholarship together, and questioning the tacit and not-so-tacit beliefs upon which they rest, can help us reframe the complex passages from empires to successor states, free

I want to extend my thanks to Howard Adelman, Steve Aron, Tom Bender, Graham Burnett, Jorge Cañizares-Esguerra, Josep Fradera, Roy Hora, Dina Khapaeva, and Rafe Blaufarb for their suggestions on this article, and to the *AHR*'s thoughtful reviewers and editors. Versions of this essay were presented as papers at the Universidad San Andrés in Buenos Aires, Smolny College in St. Petersburg, Russia, and the University of Texas at Austin.

[1] Archivo General de Indias (Seville) [hereafter AGI], Gobierno, Caracas, Legajo 458, September 13, 1806, Manuel de Guevara Vasconcelos to Principe de la Paz; September 5, 1806, Francisco Cavallero Sarmiento to Principe de la Plaz; Estado/Caracas, 71/9, November 8, 1808, "Informe de Secretaría á S.M. sobre el asunto de Miranda"; Francisco de Miranda, "Todo pende de nuestra voluntad," in Miranda, *América espera* (Caracas, 1982), 356; Karen Racine, *Francisco de Miranda: A Transatlantic Life in the Age of Revolution* (Wilmington, Del., 2003).

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from some of the teleologies of decline and triumph.[2] First, presumptions about the inevitability of imperial decline in the "age of revolutions" have cast the tensions and upheavals of the period as a sign of the sclerosis and demise of transatlantic systems, when they might better be thought of as responses to imperial adaptations. There was little that was inevitable about imperial demise. Second, revolutions were imperial in nature; that is to say, they were part of empire-wide transformations in that they yielded new social practices in defining the internal life of sovereign politics, as efforts to put empires, and their parts, on a different footing in order to confront external pressures. Revolutions did not begin as secessionist episodes; "nations" emerged as products of tensions wrought by efforts to recast the institutional framework of imperial sovereignty.

These arguments suggest a different approach to the axial shift from Atlantic empires to nation-states. They raise the prospect of altered historical sequelae, the possibility of inversions and backslidings, historic starts that went nowhere and others that never caught on despite the efforts to impose national conventions and structures on them. If the nation-state is not considered the automatic post-cursor to empire, the variety of routes, including a host of "might-have-beens," needs to be restored to the narrative about the age of revolutions. Indeed, for many corners of the Atlantic world, what emerged from imperial revolutions was not the antithesis to empire, but the revitalization of the notion of empire itself; to many contemporaries, the nation did not necessarily define itself in opposition to empire. There was a sense of the politics of imperial revolutions, their "chain of disequilibria," which was more important than the cohering nationalist drive to bring an end to empire. In the age of imperial revolutions, events and their meanings were not so easily compressed into a notion of historical time that yielded to the emergence, if not triumph, of nations.[3]

THE FATE OF EMPIRES AND THE MAKING OF NATIONS in the age of revolutions were central to the field of "Atlantic history" from the very start of its own formation as a field of scholarship seeking to transcend idiographic, nationally bound narratives about the rise of the "West." When R. R. Palmer composed his classic two-volume *The Age of Democratic Revolution* about the forty-year epic culminating in Napoleon's defeat, he argued that these upheavals were essentially democratic, which he defined as signifying "a new feeling or kind of equality, or at least a discomfort with older forms of social stratification and formal rank." The transformation was sweeping enough for Palmer to range from Poland to Pernambuco while rendering these changes in the singular, as a transformation in the makeup of the Atlantic world driven by an overreaching logic. Here was a "movement" against the possession of government by enclosed cliques of privileged men, an oceanic uprising of democrats against aristocrats. Palmer sought to transcend entrenched, exceptionalist, national narratives; for good reason, he is seen as one of the founders of Atlantic history. Now

[2] An important intervention is Frederick Cooper, *Colonialism in Question: Theory, Knowledge, History* (Berkeley, Calif., 2005), esp. 156–157.

[3] On the notion of inverted sequences, see Albert O. Hirschman, *The Strategy of Economic Development* (New Haven, Conn., 1958), esp. chap. 4 and the discussion of the "chain of disequilibria."

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that the Cold War is over, Palmer has been recovered for a post-nationalist, post-socialist turn in history—transnational, Atlantic, global, or worldwide in scale, in keeping with the temper of the times, which sees the spread of liberal democracy as the dominant tidal process.[4]

Yet Palmer's efforts to plot a narrative that escaped the fastened grip of national destinies, to evoke the age of revolutions as more than just the expression of the unique insight of "founders" or *philosophes*, relied on unquestioned assumptions about nationhood: that it was the sequel to empire once the force of democracy erupted onto the stage. Nationhood was the evolved form, the only form, in which democracy could realize itself, because nations were the peerless bounded units in which co-members could acknowledge the equal rights of others, which lay at the theoretical core of democracy, liberalism, and the kind of civic nationalism that Palmer and others extolled.[5] The assumptions about nationhood had a corollary about the *anciens régimes* they replaced: these were aristocratic monarchies whose legitimacies had dwindled because their moral foundations lost ground to intellectual and social changes. Less important to the epic was the imperial state. One is tempted to conclude that Palmer was less interested in the state, because his argument was so dependent on seeing the nation as the sequel to realize democratic forces where autocracy once ruled that he did not consider the problem of sovereignty, whether imperial or national, to be worth much ink-spilling. His narrative of the French Revolution skips the colonies, mercantilism, and slavery. The spread of abolitionism and the assault on the slave trade are spectral, and the inter-imperial conflict that ignited the fiscal crises of the regimes was imperceptible to the analysis except as background.

Almost a half-century later, our perspective on Atlantic empires looks very different. It would be hard to imagine how one would narrate their stories without placing the slave trade, slave labor, and the explosive struggles for emancipation at the center. By the same token, the work on early nationalism has revealed just how "constructed" and labored it was. The very turn to Atlantic history that Palmer sought to motivate has exposed some older presumptions about imperial arrangements that thrived on legalized systems of privilege and legitimated regimes of inequality.[6] Up to a point. David Armitage's suggestive outline of three approaches to Atlantic history (cirum-, trans-, and cis-Atlantic formulations) transcends particular national boundaries—but he admits that most of what might fall into each of these domains has been bound by particular legal or linguistic contours. In an *AHR*

[4] R. R. Palmer, *The Age of the Democratic Revolution: A Political History of Europe and America, 1760–1800*, 2 vols. (Princeton, N.J., 1959), 1: 4; Bernard Bailyn, *Atlantic History: Concept and Contours* (Cambridge, Mass., 2005), 24–30. For a neo-Palmerian take, see Lester D. Langley, *The Americas in the Age of Revolution, 1750–1850* (New Haven, Conn., 1996).

[5] This, of course, created a problem for liberals who wanted theirs to be a universal creed to transcend history, culture, and race, to be the ideology of a concert of interest-swapping nations, giving rise to an imperial liberalism that could cover its own exclusionary tracks by making judgments about the conditions for people to enjoy rights. See Uday Singh Mehta, *Liberalism and Empire: A Study of Nineteenth-Century British Liberal Thought* (Chicago, 1999).

[6] Robin Blackburn, *The Overthrow of Colonial Slavery, 1776–1848* (London, 1988); Laurent Dubois, *A Colony of Citizens: Revolution and Slave Emancipation in the French Caribbean, 1787–1804* (Chapel Hill, N.C., 2004). Some may argue that privilege and inequality were not unique to emporia, and reproduced themselves within nations. The argument here is simply that empires defined legal hierarchies in spatial terms in ways that nations did not.

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Forum in 2000 titled "Revolutions in the Americas," separate empires were the subject of discrete articles, and the synthesis by Jaime Rodríguez O. emphasized the degree to which each exemplified how empires were sundered by civil wars within them, not conflagrations between them. This argument toppled the longstanding view that colonists were nation-builders in waiting, but it did less to reconsider the larger imperial context in which state sovereignty was made—and unmade.[7] There have been notable exceptions to this view: Fred Anderson's *Crucible of War* locates the struggle of the Seven Years' War in British North America within a broader global clash that tied the fate of the Hudson Valley to Manila and Madras, not to mention Montreal. Exploring more closely the "American" Revolution, Alan Taylor examines the struggle from various positions "beyond the line"—indigenous and infra-colonial (so that French and loyalist colonists, among others, appear as more than afterthoughts in the saga). David P. Geggus and others have tracked the virus of slave insurrection beyond Saint-Domingue after 1791.[8] On an even broader scale, John H. Elliott's *Empires of the Atlantic World* is a notable study of how competing empires also looked chronically at each other. More than two decades ago, Peggy Liss compared what she called "Atlantic revolutions." For the most part, however, American, French, and even Iberian revolutions have been narrated as the product of autonomous impulses unfolding within their borders, as basic rigidities of sprawling and overextended regimes that gave way to insurrections that toppled them, most especially in the inaugural upheaval of 1776, whose historiography stamped so much of how we have come to understand empires, revolutions, and nationhood.[9]

One reason that nationally bound narratives of emerging statehood still prevail is the recurring ways in which imperial fates are told. The fact is, most histories of empires have tended to dwell on their rise and capabilities, and less on their demise or crises—in part because empires have tended to be seen as fated to conform to inevitable laws of motion.[10] When Edward Gibbon published *The History of the Decline and Fall of the Roman Empire* in 1776 (and how much he worried about Ireland and America is a matter of some speculation), he wound down chapter 49 by observing how empires violated basic natural laws: "There is nothing perhaps more adverse to nature and reason than to hold in obedience remote countries and foreign nations, in opposition to their inclination and interest."[11] In this spirit, historians and social scientists have sought to inscribe narratives of adventurous rise and eventual

[7] David Armitage, "The Concepts of Atlantic History," in Armitage and Michael J. Braddick, eds., *The British Atlantic World, 1500–1800* (London, 2002), 15–24; Jack P. Green, "The American Revolution," *American Historical Review* 105, no. 1 (February 2000): 93–102; Franklin W. Knight, "The Haitian Revolution," ibid., 103–115; Virginia Guedea, "The Process of Mexican Independence," ibid., 116–130; Jaime E. Rodríguez O., "The Emancipation of America," ibid., 131–153. There is little inter-visibility between these case studies.

[8] Fred Anderson, *Crucible of War: The Seven Years' War and the Fate of Empire in British North America, 1754–1766* (New York, 2000); Alan Taylor, *The Divided Ground: Indians, Settlers, and the Northern Borderlands of the American Revolution* (New York, 2006); David P. Geggus, ed., *The Impact of the Haitian Revolution in the Atlantic World* (Columbia, S.C., 2001).

[9] John H. Elliott, *Empires of the Atlantic World: Britain and Spain in America, 1492–1830* (New Haven, Conn., 2006); Peggy K. Liss, *Atlantic Empires: The Network of Trade and Revolution, 1713–1826* (Baltimore, 1983).

[10] This is most intriguingly argued by Charles S. Maier, *Among Empires: American Ascendancy and Its Predecessors* (Cambridge, Mass., 2006).

[11] Edward Gibbon, *The History of the Decline and Fall of the Roman Empire* (New York, 2000), xxvii as cited in introduction by David Womersley.

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fall, to be followed by successors. There is a resilient convention of telling stories about imperial decline as inevitably dictated by the form in which imperial sovereignty took, as metropoles ruling oppressed "others." If there is an imperial demise, it is inscribed in the ways these "others" resist, rebel, and defect, reducing empires to metropoles that then—albeit reluctantly—refashion themselves as nations like their offspring.[12]

WHAT REMAINS UNCLEAR IS *HOW* TO CONNECT histories of empires and nations once they are not bound by basic internal logics. These struggles unfolded in particular institutional settings—colonies as parts of empires that assembled the components of eighteenth-century notions of sovereignty. What distinguished empires was not their absolute definitions of sovereignty, but, as Lauren Benton has shown in her study of legal pluralism across a variety of colonial contexts, their amalgamation of a variety of institutional practices and their incomplete territorial contours.[13] Resetting the notion of sovereignty from a view that rests on self-evident principles of rulership, especially as regimes sprawled overseas, into one that constituted an unstable and shifting assortment of understandings and practices alters the relationship between empire and territory. It has been a commonplace to identify bounded territorial spaces with sovereignty, and therefore the nation-state with the lines that demarcated rightless from rightful subjects. This is now changing, giving way to a view of sovereignty released from *the* bounded state, and recasting it as a bundle of claims, images, and assertions of authority that can be aggregated at more than one juridical level. This new view takes some distance from the anachronism of identifying national self-determination as the modern genesis of sovereignty, and restores an appreciation for the premodern roots of our transnational political vocabulary. As we contend with the challenges of globality, it may help to reconsider the pluralist foundations of our understandings of state authority, and their ties to the boundedness of geographically distinct polities—in effect, the multiple ways in which state power and state independence have been combined and recombined over time and the fundamental disequilibrium that lies at the core of sovereignty.[14]

[12] Paul Kennedy, *The Rise and Fall of Great Powers: Economic Change and Military Conflict from 1500 to 2000* (New York, 1988); Alexander J. Motyl, *Imperial Ends: The Decay, Collapse, and Revival of Empires* (New York, 2001); David B. Abernethy, *The Dynamics of Global Dominance: European Overseas Empires, 1415–1980* (New Haven, Conn., 2000). More generally, on how regimes ossify, see Mancur Olson, *The Rise and Decline of Nations: Economic Growth, Stagflation, and Social Rigidities* (New Haven, Conn., 1982).

[13] Lauren Benton, *Law and Colonial Cultures: Legal Regimes in World History, 1400–1900* (New York, 2002); Cooper, *Colonialism in Question*, 197.

[14] Robert Jackson, *Sovereignty: The Evolution of an Idea* (Cambridge, 2007); James J. Sheehan, "The Problem of Sovereignty in European History," *American Historical Review* 111, no. 1 (February 2006): 1–15; Saskia Sassen, *Territory, Authority, Rights: From Medieval to Global Assemblages* (Princeton, N.J., 2006). Most work on sovereignty has been done by political scientists, mainly concerned with international relations. See Daniel Philpott, *Revolutions in Sovereignty: How Ideas Shaped Modern International Relations* (Princeton, N.J., 2001); and on sovereignty as resting on contradictory principles and practices, see Stephen D. Krasner, ed., *Problematic Sovereignty: Contested Rules and Political Possibilities* (New York, 2001). Christopher J. Bickerton, Philip Cunliffe, and Alexander Gourevitch, eds., *Politics without Sovereignty: A Critique of Contemporary International Relations* (London, 2007). On sovereignty and *imperium*, see Anthony Pagden, *Peoples and Empires: A Short History of European Migration, Exploration, and Conquest, from Greece to the Present* (New York, 2001), xvi–xxv.

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The shifting identification of empire with territoriality set the context for the ways in which the meaning of sovereignty itself changed in the run up to—and as a cause of—revolution. Empires did not start out with ambitions of territorial indelibility. In the wake of the Treaty of Tordesillas (1494), the pope-brokered arrangement that carved up the claims to new discoveries, the governments of Madrid and Lisbon embarked on a path of halting and incomplete—but nonetheless increasing—dominion, ruled by a king and embodied in laws that would prevail over his subjects in the form of deliverance of justice. Only with time did sovereignty become associated with territory. The same might be said when the later European claimants in the New World put down stakes: the outer boundaries of the governable hinterlands were never as clear as the mandate to bring laws to and enforce them against those under the monarch's reign. Empires spread European notions of sovereignty to distant shores with less concern for legal homogeneity up to the definable boundaries of empire. They were rather more polyglot and vague, exemplified by the prominence of gray zones that shaded the incomplete and contingent reach of empires—gray zones that would eventually evolve into borderlands. With the growing density of the imperial presence in the Americas, however, and for some areas of Africa, including the Bights and Angola, friction mounted where empires bumped up against each other; borderlands became bleeding grounds as the carnage between Europeans, Indians, Africans, and peoples "in between" intensified. In this sense, territoriality *acquired* increasing importance, and was grafted onto earlier notions of sovereignty associated with the juridical authority of the monarch.[15]

As a mechanism for state-building in Europe, empire was also deployed by the newcomers, Dutch, French, and English—who in similar fashion evolved from Atlantic foragers to actors in more integrated systems in Europe. By the eighteenth century, mercantilist empires were jockeying for supremacy from the St. Lawrence to the River Plate. Linda Colley's study of a "British" identity born out of a plurality of regional kingdoms makes it clear that global conflict with rival empires gave a metropolitan power a sense of itself as an entity united against (mainly Catholic and pope-loving) others. In the spiral of mercantilist wars, especially after the Treaties of Westphalia (1648) settled some of the basic boundary disputes in (Western) Europe, Old World states displaced their rivalries to conflict on the high seas or skirmishes over colonial borderlands—which redoubled the significance of territoriality, followed by the proliferation of efforts to define boundaries through treaties in the eighteenth century.[16]

The accent on territoriality intensified the disequilibrium and the outward expansion of European states. Empires had crises, but not because they could not adapt; it was not so much the refusal to change as change itself that yielded to the tensions of empire. By the middle of the eighteenth century, rulers and ministers were wrangling over how to adapt their ways and embark on increasingly ambitious

[15] Jeremy Adelman and Stephen Aron, "From Borderlands to Borders: Empires, Nation-States, and the Peoples in Between in North American History," *American Historical Review* 104, no. 3 (June 1999): 814–841; for a view of territorializing sovereignty from one side, see Barbara E. Mundy, *The Mapping of New Spain: Indigenous Cartography and the Maps of the Relaciones Geográficas* (Chicago, 1996), and for another side, see Peter Sahlins, *Boundaries: The Making of France and Spain in the Pyrenees* (Berkeley, Calif., 1989).

[16] Linda Colley, *Britons: Forging the Nation, 1707–1837* (London, 2003), esp. chap. 8.

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plans to modify the institutions, private and public, that held their emporia together. The efforts by Parliament to revamp the status of monopoly trading companies and transform the fiscal instruments of empire to connect the "parts" more directly in the service of the "whole" are well known to Anglo-American readers; to some extent, the same is true of the ministers and advisers of Paris's *ancien régime* who coined the very term "mercantilism" as a label for policies of imperial sovereignty. It was not by sheer coincidence that Madrid and Lisbon did the same. Variously described as the Bourbon (for Spain) or Pombaline (for Portugal) reforms, they had multiple means to pursue a broad objective: to reconstitute the empires so that private rents and public revenues flowed more effectively to support and defend the territorial contours of imperial states. Not all policies cohered. Some were more effective than others. And some were stopgaps dressed up as the brainchildren of a broad vision. But the commitment to reform cannot be denied. As Josep M. Fradera and John H. Elliott have argued, the examples of reform in Iberia were emulated by the British after the debacle of the 1770s precisely because they had been required to reform in order to catch up.[17]

Reform recombined important aspects of empires—and gave them enough stamina to suggest that predictions of their inevitable fall, and certainly their impossible adjustment, were at the very least premature. For the Iberians, the shifts were more dramatic because old ways were more deeply ingrained, but the contrasts are more in degree than in kind. Lumbering convoys were suspended in favor of licensing systems for trading ships. Viceregal habits ceded space to a multitude of new legal districts and officers, further pluralizing the spatial layers of public authority. Scarcely patrolled frontiers were militarized and fortifications built, and militias were trained, consisting of plebeian colonial populations, often free blacks and mulattos. Meanwhile, to pacify unruly gray zones, "treaties" were signed with Indian borderlanders, even by those powers such as Spain that were unaccustomed to this legal convention. Indeed, whole new practices of trading, gifting, and treating were devised—without entirely doing away with earlier ways of "reducing" Indians—to create networks of allies on the borderlands of empires, especially where those empires abutted rivals. The motivation for change was defensive, of course—a reaction and response to the compound pressures of rivalry with other empires and with each other. Each empire set about to delimit and defend the territorial reach of its domain, and within each to promote commerce, more investment in mining, settlement of frontiers, and the surge in traffic in African slaves to create a substratum of laborers upon whose shoulders the fate of empires would rest.[18]

Reconstituting the regimes provoked a riptide of opposition, less to the principles and more to the practices of new imperial sovereignty, both within the metropoles and more ominously in the colonies. From the 1760s, British colonists and their

[17] Josep M. Fradera, *Colonias para después de un imperio* (Barcelona, 2005); Elliott, *Empires of the Atlantic World*, esp. chaps. 10–11.

[18] David J. Weber, *Bárbaros: Spaniards and Their Savages in the Age of Enlightenment* (New Haven, Conn., 2005), chap. 5; John Fisher, *Commercial Relations between Spain and Spanish America in the Era of Free Trade, 1778–1796* (Liverpool, 1985); Jorge M. Pedreira, "From Growth to Collapse: Portugal, Brazil, and the Breakdown of the Old Colonial System (1760–1830)," *Hispanic American Historical Review* 80, no. 4 (2000): 839–864; Kenneth Maxwell, *Pombal: Paradox of the Enlightenment* (Cambridge, 1995); Stanley J. Stein and Barbara H. Stein, *Apogee of Empire: Spain and New Spain in the Age of Charles III, 1759–1789* (Baltimore, 2003).

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commercial patrons in Britain bridled at the flurry of commercial and fiscal demands. But these were hardly unique. Pombal ran into resistance in Lisbon, which got fierce when his patron, King José I, died in 1777—leaving the minister exposed to his many detractors. In Madrid, bread riots brought an end to experiments in free grain trade. In the Americas, there was even more unrest. The Túpac Amaru revolt in the Central Andes, Comunero uprisings farther north, seditious activity across New Spain, and the aborted Tiradentes movement in Brazil, all exemplified the various ways in which colonial peoples saw the reforms as being effective enough to disturb tacit and not-so-tacit colonial pacts among peripheral rulers and between them and subaltern agents.[19]

Imperial sovereignty was not so narrowly confined that other roads to the same ends could not be devised—or stumbled upon. In many respects, the stalemate in the fiscal struggle has masked the ways in which empires nurtured other means to harness possessions to fuel their rivalries. As the eighteenth century unfolded, the French, British, and Iberian empires contrived measures to combine labor more thoroughly with natural resources. Inter-colonial, infra-imperial, and even inter-imperial trade boomed as indigenous peoples were inducted into the market for wares and workers. From the draft of Andeans into textile *obrajes* to the luring of Choctaws into a cycle of consumption and debt, expanded territoriality had its flip side in the deepened exploitation of native peoples. But where reforms to build new economic pillars of empire were most evident was in the harnessing of African supplies of labor with American demand, with merchant capital—increasingly based in New World ports from Baltimore to Buenos Aires—serving as the source of credit for this emboldened intercontinental integration. Throughout the "age of revolutions," and despite the scare of the secession of the thirteen colonies, the traffic in African captives rose, and did not fall. Indeed, it spiked *after* the insurrection spread across Saint-Domingue and in spite of the heightened abolitionist campaigning in Europe. Some of this was fueled by the buoyed output from silver mining in New Spain and the Andes, which disgorged more specie into circulation in the Atlantic world. Some was fueled by loosened restrictions on slave trading. The old *asiento* contract that regulated the slave trade to Spanish American ports was defunct, and by 1789 Madrid issued a series of decrees opening the slave trade to individual merchants. Similar deregulations lifted restrictions on Brazilian control over trade with Africa—so much so that Rio de Janeiro was becoming the hub for a South Atlantic circulation of commodities and specie from South American hinterlands for exchange along the African coast, and deep into Angola. Consider the following numbers: From 1781 to 1790, 754,000 Africans were imported to the Americas, of whom 319,000 were destined for Saint-Domingue, which meant that 434,000 were spread across the rest of the hemisphere. The following decade saw a dip to 687,000 captives shipped, but only 66,000 bound for Saint-Domingue (leaving 621,000 for the rest of the Americas to exploit). And from 1801 to 1810, no slaves went to Saint-Domingue, but 609,000

[19] Kenneth R. Maxwell, *Conflicts and Conspiracies: Brazil and Portugal, 1750–1808* (Cambridge, 1973), 23–28, 67–71; Anthony McFarlane, "Rebellions in Late Colonial Spanish America: A Comparative Perspective," *Bulletin of Latin American Research* 14, no. 3 (1983): 313–338; Eric Van Young, "The Age of Paradox: Mexican Agriculture at the End of the Colonial Period, 1750–1810," in Nils Jacobsen and Hans-Jürgen Puhle, eds., *The Economies of Mexico and Peru during the Late Colonial Period, 1760–1810* (Berlin, 1986), 64–90.

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went to the rest of the hemisphere. And after 1808, of course, United States ports dropped off as importers of Africans.[20] The result was an increasingly autonomous and lucrative business that expanded the pool of commercial rents into which imperial authorities could dip for revenues, and a powerful class of merchant capitalists in the colonies to whom monarchs and ministers could turn for loans and loyalties.[21]

Adaptation made the empires internally heterogeneous, while in some ways making them more externally alike. The challenge was to balance the diversity within empire with the fiscal hunger and need to direct funds to metropolitan governments locked in intractable conflicts with rivals.[22] It is true, the American Revolution and the spasm of insurrection down the Andean spine were wakeup calls. But these threats did not in any way diminish the affiliation of sovereignty with the defense of empire. If anything, they were opportunities to reconstitute relations between the parts of empires. While warfare issued its blows, it also presented opportunities. For the British, as P. J. Marshall has shown, the effort to extend sovereignty in the eighteenth century may have provoked colonial unrest in the thirteen colonies, but it consolidated a hold over Indian provinces and preserved its claims in the West Indies. "Territorial empire had survived and was quickly to resume its growth" in part because the pacts that rulers learned to make with colonial mediators were so effective at striking a compromise between the illusion of unequivocal imperial authority and an admixture of systems of sovereignty at the local and regional level. Needless to say, India would prove to be the fount for empire that British North America never was. In the same manner, global war did more to recast empire in Iberian domains than to sunder them. When the 1790s sucked Spain and Portugal into the maelstrom, both regimes faced spikes in defense costs, but adapted in ways that reintegrated the parts of empires around a new matrix of slavery, silver, and de-centered sovereignty. In a way, we see Iberian authorities at the time recalibrating colonial pacts in the same ways that the East India Company authorities renegotiated the alliances between the firm and its local mediating allies.[23]

[20] David Eltis, *Economic Growth and the Ending of the Transatlantic Slave Trade* (New York, 1987), 247.

[21] Jorge Gelman, *Campesinos y estancieros: Una región del Rio de la Plata a fines de la época colonial* (Buenos Aires, 1998). Once upon a time, this pattern reinforced the view of persistent feudality of Iberian America, that this double transformation was simply a veil over patrimonial property. Richard White, *The Roots of Dependency: Subsistence, Environment, and Social Change among the Choctaws, Pawnees, and Navajos* (Lincoln, Neb., 1988); Richard L. Garner, "Long-Term Silver Mining Trends in Spanish America: A Comparative Analysis of Peru and Mexico," *American Historical Review* 93, no. 4 (October 1988): 898–935; Matilde Souto Mantecón, *Mar abierto: La política y el comercio del consulado de Veracruz en el ocaso del sistema imperial* (México, 2001); Fisher, *Commercial Relations*, 61–77; John Fisher, *Trade, War, and Revolution: Exports from Spain to Spanish America, 1797–1820* (Liverpool, 1992), 54–62; Sheila de Castro Faria, *A colônia em movimento: Fortuna e família no cotidiano colonial* (Rio de Janeiro, 1998); Manolo Garcia Florentino, *Em costas negras: Uma história do tráfico atlântico de escravos entre a Africa e o Rio de Janeiro* (Rio de Janeiro, 1993).

[22] As Carlos Sempat Assadourian has observed about an earlier conjuncture, it was possible to reconcile multiple pressures within the social and economic "spaces" of empire—and a juxtaposition of spatial responses within empires to the same conjuncture. Assadourian, *El sistema de la economía colonial: El mercado interior, regiones y espacio económico* (México, 1983), 15–16.

[23] P. J. Marshall, *The Making and Unmaking of Empires: Britain, India, and America, c. 1750–1783* (New York, 2005), 5; Carlos Marichal, *La bancarrota del virreinato: Nueva España y las finanzas del imperio español, 1780–1810* (México, 1999); Jeremy Adelman, *Sovereignty and Revolution in the Iberian Atlantic* (Princeton, N.J., 2007), chap. 3.

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ALL OF THIS MEANS THAT WE MUST ASK SOME BASIC QUESTIONS about the alleged rigidity of empires, and therefore how foreordained were their successors as sovereignty was remade in the age of revolutions. If empires were not doomed, condemned to succumb to an iron law of "overstretch," or fated to fall victim to their own inelasticities, how are we to think about the crises that they *did* plunge into? One of the implications of the preceding summary is that we cannot point to increasingly outdated notions of sovereignty as the source of the problem, as if imperial sovereignty were contained within a mold out of which it could not change or adapt. Instead of a fossilized balance of archaic forces waiting to be overturned by more robust ones (a stock-in-trade account of revolution), a spoils-driven rivalry and feverish adaptation motivated the chain of disequilibrium that destabilized the global system. Indeed, frontier expansion, intensified reliance on coerced social relations to pump commodities through Atlantic trade networks, and more war were an (explosive) bonanza. The breakdowns did not occur as prophesied (by Gibbon and other figures of the Enlightenment), as first afflicting what were seen as the most backward of empires, the Iberians, because they could not accommodate new principles of enlightened freedom. It was the Spanish reformer Gaspar Melchor de Jovellanos who observed that the breakup of the Spanish Empire was a civil war contained within and unleashed by a broader, global conflagration. It was the latter that begat the former, not just in the putatively backward and weakest of empires, but across all of their rivals.[24]

The 1790s were a turning point, in the sense that this decade accentuated prior developments while twisting imperial histories onto a new track from which it would become more difficult to deviate. With the outbreak of the French Revolution and the advent of total war, the cycle of conflict between empires ramped up. The French Republic did not divest itself of its colonies in the name of propagating freedoms; monarchy was challenged, but not empire. Instead, it clung to imperial ways, as slaves and free blacks in Saint-Domingue learned, and as autonomists in Guadeloupe also discovered, because Paris, for symbolic and material motives, could not imagine its centrality in a nation without an empire to embellish it. The difference about the "total war" after 1791 (to borrow an apt image of David Bell's to depict a fundamental change in the nature of organized armed conflict as an all-encompassing struggle of annihilation) was that victory was no longer limited to contested borderlands on the fringes, but extended to how regimes would be ruled at home and abroad. The future of monarchy itself, the colligative emblem of imperial sovereignty, was at stake, which ratcheted up the conflagration between rivals—and in turn folded the carnage between empires into a civil war within them. What Jovellanos observed of Spain could have been anticipated in the total war unleashed in the colony of Saint-Domingue.[25]

So interlocking were the rivalries that intensified competition could brook no bystanders. At first blush, Madrid and Lisbon sought to stay out of what seemed to be the continuation of an essentially Anglo-French contest. In fact, both courts

[24] Cited in José Alvarez Junco, *Mater Dolorosa: La idea de España en el siglo XIX* (Madrid, 2001), 120–121.

[25] Laurent Dubois, "The Promise of Revolution: Saint-Domingue and the Struggle for Autonomy in Guadelupe, 1797–1802," in Geggus, *The Impact of the Haitian Revolution*, 112–133; David A. Bell, *The First Total War: Napoleon's Europe and the Birth of Warfare As We Know It* (Boston, 2007).

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feuded over their allegiances, between pro-English and pro-French factions, a match that grew increasingly bitter as the revolutionary wars gave way to Napoleon's continental and Atlantic-wide ambitions. What Spain and Portugal had as their lifelines were outposts that could help compensate with rents and revenues to cover the costs of intensified rivalry over European and Atlantic supremacy. This was also, of course, their vulnerability when the sea-lanes were severed. Wartime adaptations had important effects on the internal balancing of empire and the relationship between its parts. Dependency on the fringes gave local brokers an important role in maintaining the delicate equipoise within empire that was being ravaged by disequilibrium between empires. Increasingly, the primary sites in the governance of colonial affairs were the merchant guilds and municipal councils. Viceroys and high courts still weighed in, but there was a notable devolution of power to the delegated authorities of colonial ruling blocs and their assemblies of local potentates. While much has been made of figures such as Mexico's Servando Teresa de Mier or even the adventurer Miranda as apostles of independence, colonial deliberations were dominated by loyalty to monarchy and empire, as exemplified by the concerned voices of José Ignacio de Pombo and José da Silva Lisboa.[26]

It is too simple to describe the age of revolutions as the by-product of internal and ineluctable crises of the *anciens régimes*, for within it lay opportunities to rearrange the practices of mercantilism and the terms of the exchanges between private magnates in colonies and monarchs in the metropoles—even, and perhaps especially, at the apex of the crises. Colonial loyalists advocated accelerated imperial adaptation and accommodation to new commercial realities (including more open trade) to contain the spread of political convulsion and slave unrest. This does not mean that such reforms dissolved the prospect of new frictions; they clearly sprang from aspirations to redefine imperial sovereignty and to create more institutional latticework that would reintegrate their multiple parts of increasingly de-centered empires. There was a simultaneous process of reassembling parts while giving them more, albeit partial, autonomy during the revolutionary conjuncture—which made governance complex, but not futile. Even where insurrection coursed through transatlantic sinews, as in France's or Britain's empires, these were hardly feeble and brittle regimes lying in wait for the last blow to bring them down; what is more, the insurgents were more concerned with defending rights within empire than the right to defect from it. The conventional story organized around a narrative of "origins," which points to resistances to integration as precursive to secessionist struggles, conflates the tensions associated with reassembling imperial parts and pacts with evidence of unavoidable demise.[27]

One of the reasons why the elasticity of empires has often been overlooked in favor of depictions of their essentially decrepit natures is that empires are usually

[26] José Ignacio de Pombo, *Comercio y contrabando en Cartagena de Indias* (1800; repr., Bogotá, 1986); José da Silva Lisboa, *Observações sobre a prosperidade do estado pelos liberaes principios da nova legislação* (Rio de Janeiro, 1810); Gabriel B. Paquette, "State-Civil Society Cooperation and Conflict in the Spanish Empire: The Intellectual and Political Activities of the Ultramarine *Consulados* and Economic Societies, c. 1780–1810," *Journal of Latin American Studies* 39, no. 2 (2007): 263–298.

[27] John Lynch, "The Origins of Spanish Independence," in Leslie Bethell, ed., *The Cambridge History of Latin America*, vol. 3: *From Independence to c. 1870* (Cambridge, 1985); David Brading, *Los orígenes del nacionalismo mexicano* (México, 1983).

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understood as made up of a "center" radiating out to "peripheries" that necessarily grow apart, destined to orphan their parents at the first opportunity. They are seen less as products of relationships between constituent parts, in large measure because early nationalist sources preferred to see these ties as basically inflexible sources of domination, not accommodation or negotiation. This has obscured the complex interweaving of opportunism and loyalism, interests and identities, that crisscrossed imperial systems. Yet, as more historians are revealing, sovereignty did not have only one layer to it, radiating outward to territorial boundaries with concentric circles of authority; it had many layers, which rearranged according to shifting structures and circumstances. Seen in this way, the age of revolutions intensified a process of imperial adaptation to the very rivalries and interstate system that they thrived on. Only retrospectively did these adaptive frictions become associated with proto-nationalist struggles, as if reactions to tensions were themselves the cause. At the time, they exposed the fissures and internal contradictions within each regime just as the process was being driven by competition between them. Furthermore, it was not a foregone conclusion that the most muscular of these regimes would endure and the weakest decompose. It may help future research to dispense with the normative language that was so central to prophetic claims that empires were doomed models of sovereignty, and subsequent nationalists' sermons about predestined success.

In the chain of disequilibria of imperial competition, conflict, and crisis, imperial sovereignty was less fated to yield a clear alternative to empire—never mind peaceable, democratic, and national surrogates. But there was nonetheless an important break in the legal systems that braced the early modern Atlantic world together. What happened? One common answer has been the birth of national consciousnesses within the colonies. Just as inevitablist accounts of imperial decline underestimate the elasticities (and perforce the legacies) of empire, the treatment of the rise of the nation-state as no less inevitable reduces the narrative of revolutionary outcomes to the degree to which models of nationhood had been allowed to mature under imperial rule.[28] Just as we have been accustomed to closing the gap between empire and nationhood in the age of revolutions by presuming that the former was doomed, it has been a longstanding assumption that components of the latter ripened as the days of empire grew numbered. At its strongest, a nationalist teleology credits the struggle for self-determination with the demise of empire, thus closing the gap completely. For the founding generation of historians writing in the middle of the nineteenth century, and whose documentary trails provided the invisible routes for Benedict Anderson's later work on "creole pioneers," an American identity was taking shape as the empires began to collapse.[29]

[28] Benedict Anderson, *Imagined Communities: Reflections on the Origin and Spread of Nationalism* (London, 1991), esp. chap. 4; E. J. Hobsbawm, *Nations and Nationalism since 1780: Programme, Myth, Reality* (New York, 1990). On the formation of national identities more generally in colonial societies, see Nicholas Canny and Anthony Pagden, eds., *Colonial Identity in the Atlantic World, 1500–1800* (Princeton, N.J., 1987), where identity precedes independence, and national identity emerges from colonial resistance to empire.

[29] José Manuel Restrepo, *Historia de la revolución de la República de Colombia*, 6 vols. (1827; repr., Medellín, 1969); Bartolomé Mitre, *Historia de Belgrano y de la independencia argentina*, 4 vols. (1857;

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To argue, as Anderson and others have, that nations emerged out of constructed fictive bonds promoted by communicative activity frees sovereignty from some of its primordial ("self-determining") associations, but this account of national identity casts them in opposition to, and as repudiations of, empire. Nations still stand as natural sequels to empire, and given their more "modern," leveled means of imagining social norms, they are better prepared for an Atlantic world (and eventually a globalized one) of trading nation-states. David Armitage has recently taken the argument one step further. His thoughtful examination of the global diffusion of a new model of sovereignty that was announced in 1776 traces the way it was emulated and elevated as a model document of national repudiation and state-building against empire. Thereafter, quests for self-determination could point to Jefferson's words as a founding document of statehood for what would evolve into a global network of nation-states.[30]

This suggestive argument opens the way for a less "exceptionalist" narrative of state-building and imagining citizenship (as if Americans were uniquely endowed with an ability to understand individualism and legal guarantees). But does it square with evidence of the French, Spanish, and Portuguese colonies' responses to imperial crises and their eventual secession—where independence was, in the sequence of things, more often the end of a long process of decomposition and much less of a catalyst? Some might wonder along the same lines about the thirteen colonies. There is also a logical difficulty: the causal account for change owes a great deal to the presence (or absence) of necessary conditions, implying that the outcome of modern nation-states depended on the identification of actors or agents with ideas or interests associated with systems that do not yet exist—"national," "modern," or "capitalist." Indeed, there has been a long tradition of arguing that states that "failed" (starting with an image of Haiti, as Michel-Rolph Trouillot has so trenchantly reminded us) could be reduced to the weakness of national and modern identities that made their revolutions so manqué, and thus condemned them to remain enthralled to feudal, neocolonial ways.[31] The inevitability that coats the past leaves little to the passage that connected a world governed by empires with a later cycle of national state formation. Not surprisingly, the failure or success of precursor empires and successor nations was determined long before the transformation of sovereignty set in.

But if empires were not fated to collapse, wither away, or be subsumed by successors, how are we to explain the secession of the vast majority of American colonies from the metropoles without laying the upheaval at the feet of mere circumstance, or more commonly to argue that proto-national elements were itching to free themselves from European capitals, seeking the right opportunity or pretext to justify

repr., Buenos Aires, 1947); Carlos María de Bustamante, *Resumen histórico de la revolución de los Estados Unidos Mejicanos* (London, 1828).

[30] David Armitage, *The Declaration of Independence: A Global History* (Cambridge, Mass., 2007), 19, 34.

[31] Michel-Rolph Trouillot, *Silencing the Past: Power and the Production of History* (Boston, 1995). And for this reason, current neoliberals often claim that they are the harbingers of a modern Latin America. See, for instance, Claudio Véliz, *The New World of the Gothic Fox: Culture and Economy in English and Spanish America* (Berkeley, Calif., 1994). For more on circularity, see Ellen Meiksins Wood, *Democracy against Capitalism: Renewing Historical Materialism* (London, 1995), esp. 146–180.

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overturning imperial sovereignty with a national model of statehood? How are we to trace the steps between the demise of empire, without presuming its inevitability, and the emergence of something new, without presuming its inevitability, either? The examples of the Iberian Atlantic are revealing because a long stretch of time separated the moment in which the empires went into paralysis after the end of the Peace of Amiens and their final breakup in 1821–1822. This unfolding drama, between the demise of the *anciens régimes* and the emergence of successors, says a great deal about the political nature and contingencies of the passage of sovereignty, a *politics* normally left out of the analysis of nationalism's origins, in large part because nations and empires—as the privileged repositories of sovereignty in the modern world—are more often seen as objects with definable, intrinsic features that can be plotted in narratives about "rise" or "fall," and not as arrangements, constantly reassembling themselves.

Rather than assertions of national independence against empires, much more common in the complex breakdown of empires was the exploration of models of re-accommodating colonies into imperial formations, a groping for an arrangement that would stabilize, not dissolve, regimes. Colonial and metropolitan ruling classes more often discussed the management of the crisis within a framework of "loyalty" that yielded to changes in sovereignty as *processes*, with starts that led nowhere and endings that surprised even the most prescient of actors, and eventually led actors to the "exit" option—in the sense that motivated Albert O. Hirschman to argue in *Exit, Voice, and Loyalty* that people cope with crises in a variety of ways, with an "inborn tendency toward instability" of even the most perfect of mixes.[32] In the prolonged improvisation from 1807 to 1822, old systems were giving way before there was any clear sense of finding new ones, forcing historians to dispense with the discrete stages, or smooth passages, so often invoked to account for macro-social change.

The tendency to disequilibrium was embedded within an increasingly combustible interstate system, riven by warfare in the 1790s, and escalating to an epic confrontation and the shift to total Atlantic-wide war after 1805, not a brewing tempest within each empire. Indeed, the growing weakness of state structures did not provoke secessionist movements when it would have been easiest to "exit." Just as the reconstitution of empires was a response to competitive pressures of the eighteenth century, the final breakup of Iberian empires was the effect of even more heightened rivalry, not just stoked by the scramble for mercantilist possessions, but by 1800 involving a struggle for trans-regional hegemony. To this point, autonomous networks of exchange in the South Atlantic, combined with adaptations in imperial governance, fueled by an ever more vibrant slave trade, meant that declarations of loyalty to monarchy had been backed by larger pools of commercial rents. The result was a renegotiation of the pacts between colonial outposts and capitals and within the coalitions of forces in the peripheries. For the physiocratic *letrados* in Cartagena and Buenos Aires, all that was required was for authorities to turn ad hoc adjustments into a new model. This is what was transpiring in the Portuguese empire: Rodrigo de Souza Coutinho, the most influential member of the Braganza court from

[32] Albert O. Hirschman, *Exit, Voice, and Loyalty: Responses to Decline in Firms, Organizations, and States* (Cambridge Mass., 1970), 126.

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the 1790s to 1812, observed that Portugal was far more dependent on Brazil than vice versa, and by 1804 he was concocting plans to relocate the monarchy to the new metropole, in Rio de Janeiro. To be sure, there was plenty of complaining about residual monopolies and griping about restrictions on commerce, but such tyrannies and vices were hardly grounds for defection. Far from it: ridding the empires of the agents of "corruption" would revitalize them and make them more durable. New perspectives on property, in fact, bore no automatic association with a new outlook on sovereignty.[33]

The possibility of an evolution toward a fundamentally new pact between the component parts and actors in the Iberian empires was not infinitely elastic, for it was still subject to the external dynamics of interstate conflict. Yet actors on all sides tried to rethink incumbent regimes before giving up on them; renewed warfare was the last thing anyone wanted. The governments in Lisbon and Madrid faced unenviable choices as Britain and France poised to square off once more. Until Napoleon dispatched armies across the Pyrenees in 1807, there was deep disagreement in both governments over how to face the crisis. In the end, the Spanish monarchy was toppled by Napoleon's sleight of hand, and in short order the Spanish Empire was missing its keystone, prompting an upsurge in anxiety over how to govern an empire without a king. In Portugal, the French invasion simply displaced the emblem of sovereignty instead of decapitating it, as the monarchy took refuge in a new imperial capital, the erstwhile colonial outpost of Rio de Janeiro. By becoming "Americanized," the monarchy was spared the immediate question of what bound the colonies to *ancien régime* sovereignty.[34]

Striking at the core of each empire, the French armies forced the ruling cliques in each regime to reconstitute empires with improvisations to rescue them. These improvisations, resting on new practices of public representation, shook up old pacts and ruling coalitions, and revealed the belatedness of new models of representation as well as the precociousness with which they spread, compared to the British or French empires. The first was the transformation in communications. Governments in both empires lifted restrictions on the press (in Mexico, Lima, Buenos Aires, and elsewhere) or allowed a press to begin to take root in the first place (in Brazil, Caracas, Chile, and elsewhere) to re-sacralize the monarchy and rebuild confidence in its ministers. The interim government in Spain, fleeing the pursuing French armies in Andalusia, abolished the inquisition and declared an end to state censorship. Indeed, what Miranda had thought would be the decisive weapon in the war, the printing press, served initially to promote colonial loyalty to the king and Spain. With time, however, the very instrument that was unfettered to legitimate the Spanish government gave way to more and more lurid accounts of colonial administrators' misdemeanors and eventually a means to broadcast bad news from the metropole. In Brazil, the role of the press was different; the court brought the first printing press

[33] José da Silva Lisboa, *Principios de economia política* (Lisboa, 1804), 112–116; for more details, see Adelman, *Sovereignty and Revolution*, chap. 4; Kenneth Maxwell, "The Generation of the 1790s and the Idea of Luso-Brazilian Empire," in Dauril Alden, ed., *Colonial Roots of Modern Brazil: Papers of the Newberry Library Conference* (Berkeley, Calif., 1973), 107–144.

[34] Brian R. Hamnett, *La política española en una época revolucionaria, 1790–1820* (México, 1985), 57–67; Valentim Alexandre, *Os sentidos do império: Questão nacional e questão colonial na crise do Antigo Regime português* (Lisbon, 1993).

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with it to the colony with an eye toward using it as a means to promote closer ties between colonial subjects and the Braganza court. Either way, governments had to cope with the birth of public opinion; this meant that contending with dissent with inquisitorial zeal or indifference was likely to produce much more than mere consent of the governed. Of course, public opinion was free in degrees: it was more open in some corners, such as Buenos Aires and Bogotá, but was muzzled in Brazil and in Peru. But even these holdouts against unfettered public opinion eventually gave way.[35]

There was a second major effort to re-found imperial sovereignty. In an effort to recombine the parts of empire on the backs of "public opinion," metropolitan governments in Spain in 1808 and Portugal in 1820 called for constitutional assemblies to draft a founding charter of imperial nationhood to reinvigorate the ties between rulers and ruled. The Spanish Junta issued a clarion call to the colonies in the name of "the nation," insisting "that the Spanish dominions in America are not colonies, but an essential and integral part of the Monarchy." Accordingly, each part of the empire-nation was invited to elect envoys and dispatch them to a new assembly charged with drafting a founding charter. Much the same obtained later in Portugal. Electoral activity in towns across both empires sprang to life (with a few exceptions). However, when American delegates arrived at the assemblies, they immediately encountered a wall of resistance to their understandings of the equality of all subjects of the empire. Metropolitan delegates contrived ways to diminish the strength of colonial delegations—which did little to endear Lisbon and Madrid to colonial outposts. The burst of electoral activity was meant to bolster the legitimacy of the regimes, and to some extent it did. But it also had the effect of revealing the colonial status of American subjects, which until then could be mystified by the mechanisms of viceregal justice.[36]

"Modern" modes of representation and public spheres erupted suddenly within Iberian-Atlantic societies, nurtured by the struggle to define and mold public opinion in favor of reconstituted imperial nations, creating new means to re-found the social imaginary of empire. This shift occurred during a dramatic contest for local political, social, and economic resources, which drove contestants to seize opportunities afforded by new mechanisms of voice and representation. It is important to underscore that there was an enormous amount of confusion, which cannot be simplified or reduced to "revolutionary" or "counterrevolutionary" mobilization. Calls for independence were rare. For the most part, the feuding in the colonies was over how best to cope with the crisis of the empire, using new means to legitimate it precisely in order to revive it. Where protagonists pushed furthest to reimagine sovereignty, in Caracas, Nueva Granada, Michoacán, and Guerrero, announcements of home rule

[35] François-Xavier Guerra and Annick Lempérière, "Introducción," in Guerra and Lempérière, eds., *Los espacios públicos en Iberoamérica: Ambigüedades y problemas, siglos XVIII–XIX* (México, 1998), 5–21; Victor Uribe-Uran, "The Birth of a Public Sphere in Latin America during the Age of Revolution," *Comparative Studies in Society and History* 42, no. 2 (2000): 425–457; on elections in Mexico, see Guedea, "The Process of Mexican Independence"; Victor Peralta Ruíz, *En defensa de la autoridad: Política y cultura bajo el gobierno del virrey Abascal—Peru, 1806–1816* (Madrid, 2002). For a pioneering study of the late colonial press, see Renán Silva, *Prensa y revolución a finales del siglo XVIII: Contribución a un análisis de la formación de la ideología de independencia nacional* (Bogotá, 1988).

[36] *El Argos Americano*, November 18, 1811; Hamnett, *La política española*, 73–101; Jaime E. Rodríguez O., *The Independence of Spanish America* (New York, 1998), 82–91.

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led to civil war. In all cases, loyalists and their armies triumphed. When a Spanish constitution was approved by deputies in the Cádiz Parliament, it seemed a plausible framework for revitalizing empire. Meanwhile, Brazilians got home rule *de facto* because there was nothing to secede from.[37]

The idea of independence was anything but contagious. More pervasive was the internal discord, at times bloodletting, over how to reassemble the shattered parts of empire into new wholes under rapidly changing political ground rules. Iberian empires, like their rivals, were under threat in various ways by 1814, but they had not broken up. Rather than decompose, they recomposed. Save the outliers of the thirteen colonies and Saint-Domingue, loyalists had the upper hand across the rest of the Americas. There were a few cases, including the band of home-rulers in the River Plate, where secession got a toehold. But for the most part, empires had managed to reintegrate themselves in spite of the metropolitan crises of total war.

This might suggest that empires had weathered total war. Some did. Others did not. Yet others gave way to new empires with New World foundings. Tracing subsequent events reveals a great deal about the multiple fates of empires and the upheavals that sundered them or gave them new significance. Instead of one fate, there was a spectrum. Some, including the British in Canada and the Caribbean, let local councils evolve to accommodate an amalgamation of local and imperial identities. The French did much the same in Guadeloupe and Martinique. The long-term result was a loyalist framework to accommodate future challenges. At the other extreme was Spain. Ferdinand, bolstered by metropolitan merchants eager to reclaim defunct privileges, was determined to reinstate Spain's centrality in an empire that had, in the meantime, reaggregated its heterogeneity. The king launched a counterrevolution to re-center the empire by tearing up the short-lived constitution and reimagining himself as a benevolent absolutist. This had the effect of emboldening a new secessionist coalition to include many who embraced home rule within the empire and its constitution. Whereas Simón Bolívar had all but given up on his cause by 1815, Spanish revanchism gave him a new lease on life, spurring many corners, classes, and castes of colonial society to resist turning the clock back. Now the civil war between loyalists intensified, and in doing so it accelerated the mobilization and militarization of Indians, slaves, and plebian populations. By 1820, Ferdinand's ambitions were not just losing him loyalists in the colonies; unrest shot through the very pillar of authority upon which he most rested: the army. When the discontent finally broke out in military revolts in the peninsula itself, the logic of restoring the *ancien régime* collapsed, bringing down the power of the monarchy in the metropole. In turn, the crisis of the Spanish Empire engulfed Portugal, which had in fact gone the farthest to reconstruct sovereignty around a new spatial balance. Lisbon caught the Spanish bug, and insisted on restoring a version of its own centrality—yielding the same outcome. When it threatened to curb Brazil's autonomy within the empire, Brazil's ruling classes rallied behind the exit option, keeping monarchy and the notion of empire for themselves precisely to preserve the privileges of an ennobled slavocracy. Thus in general, from an Iberian-colonial perspective, there was little left to remain loyal to; the armies of empire folded up their tents, joined secessionists, or went

[37] Rodríguez, *The Independence of Spanish America*, chap. 3; Adelman, *Sovereignty and Revolution*, chap. 5; Roderick J. Barman, *Brazil: The Forging of a Nation, 1798–1852* (Stanford, Calif., 1988), 43–55.

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home. It was at this point that "declarations of independence" spread in response to the shakeup of empire at the core.[38]

An important shift in the nature of conflict over sovereignty had taken place. The effort to create a centralized system (imagined as a "restoration") had shattered the compromises of previous years. It also shifted the dynamics of local tensions, which became more and more endogenous—that is, disconnected from original causes of conflict and spiraling enough to ravage colonial coalitions. Civil war within empire took the place of total wars between empires as the source of disequilibrium. As the tensions and violence turned inward, and warfare and politics became increasingly "irregular" (in the sense that partisanship became more bellicose, and armed conflict lacked front lines), the very geographic fundaments that had been packed into the adapted notions of imperial sovereignty—that is, jurisdiction over territorial spaces up to definable limits—fractured deeply. It was these civil wars that yielded to independence, and not secession that sparked the civil war.[39]

SECESSIONS WERE RESPONSES TO CRISES OF SOVEREIGNTY, produced first by international war, and subsequently by civil war. They were not homegrown exit options maturing within empire and associated with a different model of sovereignty, announcing themselves into existence when the oppressions of empire grew too onerous or the opportunities to secede became too tempting to dismiss. Indeed, disequilibrium within empire wrought by total warfare, international then civil, toppled incumbent regimes long before successors could fill the void. This is an important point. Declarations of "independence" in the name of nations yielded to the very same propensities to breakup as the empires they rejected. It was the shift from loyalty to voice, and eventually to exit—to use Hirschman's triad—that inaugurated a search for new models of sovereignty. None of this unfolded as a natural sequence, one stage necessarily leading to the next. Indeed, during the breakup, some stages were inverted. Among many, for instance, the empires were on sounder footing in 1812 than in 1807; the French occupation of Iberia had emboldened declarations of loyalty rather than stoked the urge to secede. Elasticity of such polymorphous sovereignties, and not rigidity, gave empire more durability than historians have often credited it with. It was in fact the effort to enforce metropolitan centrality after 1815 that depleted the ranks of imperial coalitions and revived secessionist coalitions. As the Atlantic empires imploded on themselves, and the once-integrated parts broke into pieces, new understandings of sovereignty emerged, inheriting the same complex traits of regionalism, racialized labor systems, and models of representation improvised during the struggle to shore up empires.

[38] John Charles Chasteen, *Americanos: Latin America's Struggle for Independence* (New York, 2008); Margaret Woodward, "The Spanish Army and the Loss of America, 1810–1824," *Hispanic American Historical Review* 48, no. 4 (1968): 586–590; John Lynch, *The Spanish American Revolutions, 1808–1826* (New York, 1986), 212–214; Timothy E. Anna, *Spain and the Loss of America* (Lincoln, Neb., 1983); Isabel Lustosa, *Insultos impressos: A guerra dos jornalistas na independência, 1821–1823* (São Paulo, 2000); Barman, *Brazil*, 70–71; Márcia Regina Berbel, *A nação como artefato: Deputados do Brasil nas cortes portuguesas, 1821–1822* (São Paulo, 1999).

[39] On the self-sustaining mechanisms of civil war, see Stathis N. Kalyvas, *The Logic of Violence in Civil War* (New York, 2006), esp. 81–83, 88 on endogeneity.

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The same complex inter-leaving process that connected empires to the revolutions brought them down. The age of revolutions saw empires seeking to reconstitute the elements, and at times foundations, of sovereignty, with new repositories of legitimacy for public powers assembled with old ones. The combination of an enhanced royalism with greater powers to Parliament provided a source of integration as well as division in the Anglo-Atlantic world. In the French Empire, it was the promise of freedom and some autonomy within the Revolution that kept Caribbean colonies faithful to the metropole. In the end, it was Napoleon's decision to restore slavery to Saint-Domingue that divested him of support from erstwhile loyalists among the ex-slave and freed-black populations—and even they eventually proclaimed the independence of Haiti as a more virtuous *empire* able to make good on the principles of the French Revolution that the old *empire* had betrayed. What was common was the prolonged effort to reassemble the practices of sovereignty under empire—and to rely on empire to shore up understandings of sovereignty.[40] In this fashion, the narratives about transitions from colonies to nations can be recast without conventional accounts of redemptive struggles of oppressed nations seeking liberty from venal or tyrannical imperialists as the causal forces behind revolutions. At the same time, since empires were not necessarily seen as models of sovereignty doomed to fail out of their own internal limitations or contradictions, to be resolved in a higher, more perfect form in the nation-state, the struggle to resolve the crisis of imperial sovereignty once it did set in often reproduced the same intractable features of incumbents within the regimes that replaced them. Narratives of empire need not collapse repeatedly into familiar dichotomies of empire *or* nation, empire *versus* nation.[41]

What is more, many aspects of the old regimes fell by the wayside in the course of the revolutions, but empire was not always one of them—indeed, to many colonists, constituting a sovereign state required the preservation of empire and the Americanizing of a monarchy. Not surprisingly, many elites opted to preserve the one model of sovereignty that had proved so durable, and that, now shorn of its affiliation with the Old World, could be better adapted to the New one: empire. Haiti has been mentioned. Mexico under the short-lived empire of Agustín I could be added to the list. There is also the loyalty of Cuba and Puerto Rico to Spain, which endured until the late nineteenth century, or for that matter Canada's loyalism into the twentieth. All sought to adapt themselves to an evolving imperial constitutionalism. The most overlooked example, of course, has been Brazil, as empire and monarchy mutated to subsume national, capitalist, and liberal precepts within tropical regalism, as the work of José Murilo de Carvalho has shown.[42] Brazil was a variant on a larger theme about a complex transformation riddled with contingencies in which colonial subjects did not see themselves as facing idealized choices between

[40] Brendan McConville, *The King's Three Faces: The Rise and Fall of Royal America, 1688–1776* (Chapel Hill, N.C., 2006); Marshall, *The Making and Unmaking of Empires*; Knight, "The Haitian Revolution"; Dubois, *A Colony of Citizens*, 349–374.

[41] Rodríguez, *The Independence of Spanish America*; José Carlos Chiaramonte, "Modificaciones del Pacto Imperial," in Antonio Annino, Luis Castro Leiva, and François-Xavier Guerra, eds., *De los imperios a las naciones: Iberoamérica* (Zaragoza, 1994), esp. 108–111.

[42] José Murilo de Carvalho, *A construção da ordem* (Rio de Janeiro, 1996).

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verticalist empires ruled by monarchs and leveled nations in the garb of republican constitutions.

And yet, some revolutions dimmed the fortunes of imperial reconstitution. In Spanish America, this option could not contain the forces that had been unleashed as civil conflict within empire turned inward and became more irregular. As the forced labor systems became ravaged and plebian folk flocked to armies and guerrilla forces on all sides (half the foot soldiers of liberating armies were manumitted slaves), the plurality of notions of sovereignty was hard to contain in a single mold—whether national or imperial. Subaltern actors, from Indian villagers in Oaxaca to the free blacks of Cartagena, asserted their own views of government, adding to the disequilibria that pushed former colonial peoples farther from the possibility of any restored empire. Some of these understandings inhabited the very hybrid infrastructures of imperial sovereignty, only to acquire autonomous leases with civil war and revolution. Fueled by the struggle over public opinion and armed affiliations, the postcolonial dawn yielded to a spectrum of sovereignties—from municipal self-government, to federated provinces, to confederations of "states" under a shared constitutional umbrella, none of which could be packed back into the capacious, and frustratingly amorphous, entity of empire.[43]

With these counterpoints in mind, the story of the independence of the thirteen colonies resembles one, no less unstable, expression of subjects' efforts to grapple with models of sovereignty within empire. Loyalists' and monarchists' fidelity to the incumbent regime, and their reconstitution in the rest of British North America and the Caribbean, suggests that the decision over "exit" was less of a foreordained response to imperial change than one might surmise from the enduring appeal of epic-triumphal narratives. The declaration of 1776 might appear as one of a number of paths to recompose the principles and practices of sovereignty in the age of imperial revolutions. And even this demarche can be seen as repudiating one concept of empire without disbanding its durability altogether. What is more, as the conflict over imperial sovereignty mounted in the 1770s, the ambiguities and tensions within the patriotic cause itself became clearer as the social hierarchy began to shake when enslaved and plebian folk seized the opportunity to translate a contest over which empire would rule into a war over what had to be ruled.[44] The fates of the French, Spanish, and Portuguese empires might not look like such outliers to a conceit about exceptionalized origins of modern sovereignty and nationhood. Needless to say, the Louisiana Purchase rekindled dreams of a model of expansion for an internal slavocratic empire that would rescue the republic from its troubles.[45]

[43] Peter Blanchard, "The Slave Soldiers of Spanish South America: From Independence to Abolition," in Christopher Leslie Brown and Philip D. Morgan, eds., *Arming Slaves: From Classical Times to the Modern Age* (New Haven, Conn., 2006), 261–266; Marixa Lasso, *Myths of Harmony: Race and Republicanism during the Age of Revolution, Colombia, 1795–1831* (Pittsburgh, 2007); Peter Guardino, *The Time of Liberty: Popular Political Culture in Oaxaca, 1750–1850* (Durham, N.C., 2005).

[44] Michael A. McDonnell, *The Politics of War: Race, Class, and Conflict in Revolutionary Virginia* (Chapel Hill, N.C., 2007).

[45] This recasting is explored in Eliga H. Gould, *The Persistence of Empire: British Political Culture in the Age of the American Revolution* (Chapel Hill, N.C., 2000); Daniel J. Hulsebosch, *Constituting Empire: New York and the Transformation of Constitutionalism in the Atlantic World, 1664–1830* (Chapel Hill, N.C., 2005); Peter S. Onuf, "'The Strongest Government on Earth': Jefferson's Republicanism, the Expansion of the Union, and the New Nation's Destiny," in Sanford Levinson and Bartholomew Sparrow, eds., *The Louisiana Purchase and American Expansion, 1803–1898* (New York, 2005), 41–68; T.

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Thus European sovereignty need not be treated as comprising autopoietic and independent regimes, radiating out of bases or "centers" in Europe to aggrandize self-referential polities, to fringes in Africa, America, and Asia. It can be seen as part of a system with specific extrinsic features that came to a head in the late eighteenth century, creating revolutionary conditions that did not figure in the pages of Palmer's epic. In this respect, imperial histories should be treated as more than the outgrowths of the history of "European" national institutions. Ironically, this historiographic formulation had the tacit, but core, purpose of linking empire to nationhood in order to illustrate how the latter was a natural, more benevolent—indeed "democratic"—successor to the former, to resolve the contradictions of sovereignty of stratified empires by replacing them with a "concert," "community," or "league" of self-governing nations as an idyll of modernity.[46]

The histories of empire and nation were necessarily entwined as part of a broader, "global" conjuncture with systemic features premised on ramped-up competition between rival regimes that constituted an imperial-state system, an escalation that grew ever more fierce as the circulation of American silver and the trade in African captives raised the territorial stakes. This gave it enormous vitality and power, but also made it unstable, the source of chronic disequilibrium. In this context, empires did not "decline" as a result of basic laws of diminishing returns applied to regimes incapable of coping with more youthful, arriviste rivals. It was a crisis of sovereignty of and within empires that spawned social revolutions, not revolutionary claims that raised fundamental questions about state structures. To be sure, to observe that democratic energies were released in the course of the struggle, and not as causes of it, is not to presume that, once at work, democratic models of sovereignty resolved the basic paradoxes and instabilities of predecessors. On the contrary, sovereignty remained as equivocal as ever: empire and territorial aggrandizement at the expense of neighbors did not cease to be a common way to quench the recurrent drive to align territoriality with states, now justified in the idiom of the "nation" while carrying with them traits of old empires that would be projected inward. Instead we find a process in which old and new practices and understandings of sovereignty were reassembled, often in a desperate effort to prevent political order, and the social inequities that sustained it, from vanishing altogether. Among the consequences of this reframing of the "age of revolutions," historians might see more clearly how the legacies of empire and colonialism endured through, or were reproduced by, the very revolutions that were said to have brought about their ends. Imperial revolutions that gave way to successor regimes may have reconstituted political orders with new elements, but they hardly did away with many of the underlying ambiguities of sovereignty. It is possible to see in "imperial revolutions" struggles for rights and the origins of movements that continue to echo across the Americas, efforts that were not reducible to the necessary triumph of the nation-state.

We must look to open a passage between a world of empires and a world of nations that does not presume the inevitable demise of the former or the triumph

Alexander Aleinikoff, *Semblances of Sovereignty: The Constitution, the State, and American Citizenship* (Cambridge, Mass., 2002).

[46] See, for instance, C. A. Bayly, *Imperial Meridian: The British Empire and the World, 1780–1830* (London, 1989).

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of the latter. Doing so frees our understandings of the politics of sovereignty from being reduced to either logic. This, after all, is what gave the age its revolutionary tonic—that there were so many plausible futures of sovereignty. Ultimately, the implications should upset what is often framed in highly normative terms: experiments that did not aggregate into national syntheses and histories of constitutional unsettlement can be spared from the condescending status as "failed states" among ungovernable peoples, as exceptions to someone's rule about "successful" states and their inspired constitutions.

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Reforestation, Landscape Conservation, and the Anxieties of Empire in French Colonial Algeria

CAROLINE FORD

> Since the beginning of time, on the dry earth of this limitless land scraped to the bone, a few men had been ceaselessly trudging, possessing nothing but serving no one, poverty-stricken but free lords of a strange kingdom.
>
> Albert Camus, *Exile and the Kingdom*

THE HISTORICAL LITERATURE ON THE CONSTRUCTION OF EUROPEAN EMPIRES and on the ways in which colonial relationships insinuated themselves into European metropolitan life has abounded in recent years. Some of this renewed interest, which can be observed among Anglo-American scholars and historians of European colonialism, has focused on how non-European landscapes were construed, conserved, and transformed by both European travelers and colonizers from the sixteenth century to the end of colonial rule in the twentieth century. In a pathbreaking study of these questions, Richard H. Grove traces the beginnings of modern Western environmentalism to the European encounter with the non-European tropical island Edens in the Indian Ocean at the end of the seventeenth century. Diana K. Davis has recently examined European narratives of environmental decline in North Africa in the nineteenth and twentieth centuries, while David Arnold and Paul Carter have assessed European responses to unfamiliar landscapes and how such landscapes became objects of desire, fear, and mastery in India and Australia respectively. To a lesser extent, historians have also addressed indigenous responses to these European initiatives and perceptions.[1]

The multiple ideological underpinnings of forest conservation in colonial Algeria

Earlier drafts of this essay were presented at the European History Colloquium at UCLA and the French Culture Workshop at Stanford University's Humanities Center. I am very grateful for my colleagues' comments at both venues, and I owe special thanks to Sanjay Subrahmanyam for his invaluable advice. Finally, I wish to thank both Rob Schneider for his helpful comments and all of the staff of the *AHR* for their expert editorial work.

[1] For a discussion of recent literature on French colonialism, see Eric T. Jennings, "Visions and Representations of French Empire," *Journal of Modern History* 77, no. 3 (September 2005): 701–721. Richard H. Grove, *Green Imperialism: Colonial Expansion, Tropical Island Edens and the Origins of Environmentalism, 1600–1860* (Cambridge, 1995); Diana K. Davis, *Resurrecting the Granary of Rome: Environmental History and French Colonial Expansion in North Africa* (Athens, Ohio, 2007); David Arnold and Ramachandra Guha, eds., *Nature, Culture, Imperialism: Essays on the Environmental History of South Asia* (Delhi, 1995); Arnold, *The Tropics and the Traveling Gaze: India, Landscape, and Science, 1800–1856* (Delhi, 2005); Paul Carter, *The Road to Botany Bay: An Essay in Spatial History* (London, 1987). William Beinart has also traced the origins of conservationism in South Africa from the eighteenth century on. Beinart, *The Rise of Conservation in South Africa: Settlers, Livestock, and the Environment, 1770–1950* (Oxford, 2003). For a discussion of indigenous responses, see Ramachandra Guha, *The Un-*

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from the 1860s to World War I constitute an important dimension of the European encounter with colonized landscapes. While the British were, of course, also concerned with forested landscapes during the same period in colonial India, it is important to understand what made the Algerian case distinct and to shed light on how different forms of colonial domination determined differing relationships to land and landscape.[2] Forest conservation, however, was disputed among colonial administrators and military authorities, and among French and European (Italian, Maltese, and Spanish) settlers themselves, which speaks to the difficulty involved in seeing a single unified "colonial project" with respect to landscape conservation in the French Empire.[3]

A consideration of the nature and distribution of Algerian forests toward the end of the nineteenth century must precede any exploration of French anxieties surrounding deforestation, arguments for reforestation, and initiatives for environmental reform, such as measures taken by the state in 1903 to create a forest code that was specific to Algeria. The limitations of and lacunae in the sources for Algeria in this period present the historian with a formidable challenge. It is almost impossible to recover fully the "voices" of the indigenous populations, but to a limited extent, the records and archives make it possible to examine their responses to French policies and to the debate about both deforestation and reforestation.

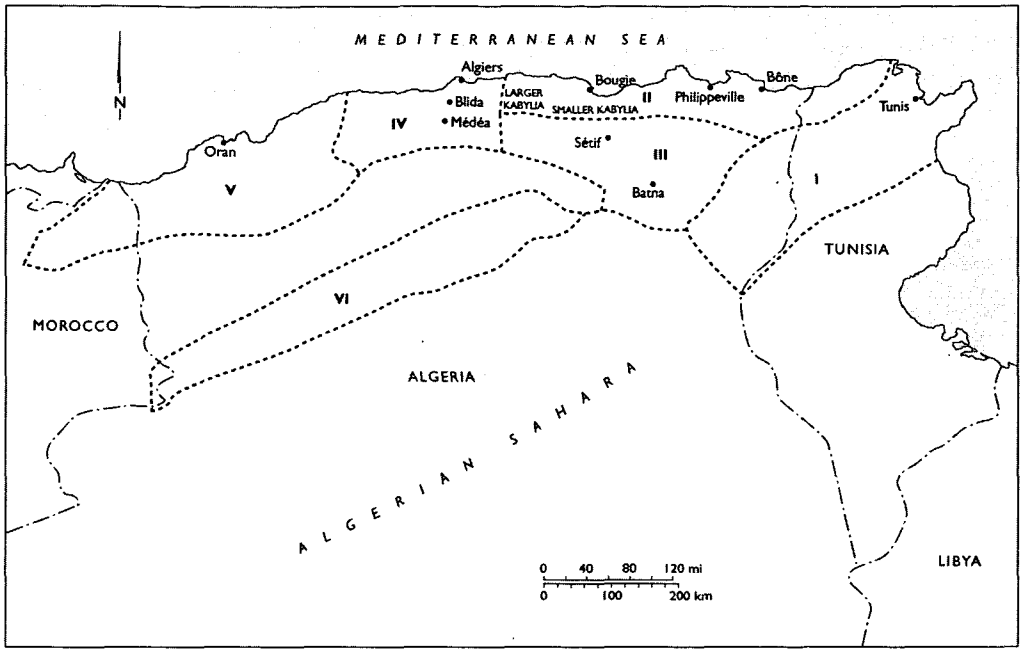
The particular form of "environmentalism" that emerged in French Algeria can fruitfully be compared to other colonial contexts. Although views differed among French settlers and colonial administrators, an imperial conception of the North African past, a settler insistence that Algeria be reclaimed as a Europeanized "Mediterranean" landscape, and France's competition with other colonial powers in Africa gave rise to conservation policies that were justified in unique ways. This environmental consciousness was shaped as much by the form that colonialism assumed in Algeria—settler colonialism—and how the French conceived their role in the Maghreb's history as by any "ecological" concern. Just as important, however,

quiet Woods: Ecological Change and Peasant Resistance in the Himalaya (Delhi, 1989); and Ajay Skaria, *Hybrid Histories: Forests, Frontiers and Wildness in Western India* (Delhi, 1999).

[2] For British policies regarding colonial forests, see, for example, Dane Kennedy, *Magic Mountains: Hill Stations and the British Raj* (Berkeley, Calif., 1996); Ravi Rajan, "Imperial Environmentalism or Environmental Imperialism: European Forestry, Colonial Foresters and the Agendas of Forest Management in British India, 1800–1900," in Richard Grove, Vinita Damodaran, and Satpal Sangwan, eds., *Nature and the Orient: The Environmental History of South and Southeast Asia* (Delhi, 1998), 324–371; and Richard Tucker, "Non-Timber Forest Products Policy in the Western Himalayas under British Rule," ibid., 459–483. Initiatives in Algeria also differed from those undertaken in other parts of the French Empire. For Indochina, see Frédéric Thomas, *Histoire du régime et des services forestiers français en Indochine de 1862 à 1945: Sociologie des sciences et des pratiques scientifiques coloniales en forêts tropicales* (Hanoi, 1999); and Thomas, "Ecologie et gestion forestière dans l'Indochine française," *Revue française d'outre-mer* 85, no. 139 (April–May 1998): 59–86.

[3] For a discussion of pluralized colonialisms, see Nicholas Thomas, *Colonialism's Culture: Anthropology, Travel, and Government* (Princeton, N.J., 1994); Ann Laura Stoler, "Rethinking Colonial Categories: European Communities and the Boundaries of Rule," *Comparative Studies in Society and History* 31 (1989): 135; and Eric T. Jennings, "From Indochine to Indochic: The Lang Bian/Dalat Palace Hotel and French Colonial Leisure, Power and Culture," *Journal of Asian Studies* 37, no. 1 (2003): 161–163. In her excellent study of narratives of environmental decline in North Africa, Diana Davis tends, in contrast, to see French environmental policy as fundamentally the same in Algeria and the protectorates of Morocco and Tunisia as well as being chiefly determined by a single project, which was to expropriate land for commodity production. Davis, *Resurrecting the Granary of Rome*, 165–166.

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MAP 1: Forested Regions of Algeria. Map drawn by Bill Nelson.

French attitudes and policies toward the environment reflected broader colonial anxieties about climate, race, and the fate of European "civilization" itself.[4]

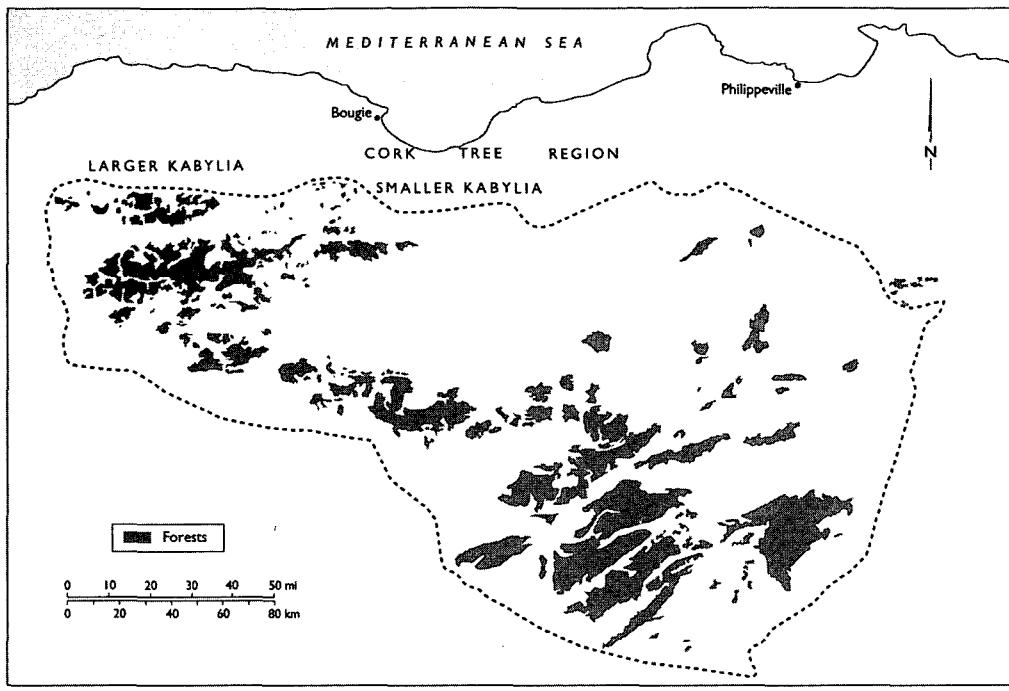
BY THE SECOND QUARTER OF THE NINETEENTH CENTURY, Algeria, which was divided into three administrative departments, had as its boundaries the Sahara to the south, Tunisia to the east, Morocco to the west, and the Mediterranean Sea to the north. Its territory could be divided into four main topographical areas: the Tell Atlas, the Hauts-Plateaux or High Plains, the Sahara Atlas, and the Sahara—and six types of forested regions: the Algerian-Tunisian region; the cork-producing region of Kabylia, which juts into a part of the north of Tunisia; the high plains of Constantine and the Aurès; the Ouarsenienne region; the Oran-Moroccan region, which included part of Morocco; and the Saharan Atlas.[5]

A variety of types of property and land tenure existed in Algeria when the French invaded, including private property (*melk*), collective, communal property (*arch*),

[4] For stimulating new reflections on the varieties of European settler colonialism, see Caroline Elkins and Susan Pedersen, eds., *Settler Colonialism in the Twentieth Century: Projects, Practices, Legacies* (London, 2005); and for some recent essays on the environmental history of settler societies, see Tom Griffiths and Libby Robin, eds., *Ecology and Empire: Environmental History of Settler Societies* (Edinburgh, 1997).

[5] Theodore S. Woolsey, *French Forests and Forestry: Tunisia, Algeria, Corsica* (New York, 1917), 47; and Paul Boudy, *Economie forestière Nord-Africaine*, vol. 4: *Description forestière de l'Algérie et de la Tunisie* (Paris, 1955), 1. See also Achille Fillias, *Etat actuel de l'Algérie: Géographie physique et politique de l'Algérie—Dictionnaire géographique et historique de toutes les localités* (Algiers, 1862); Fillias, *Notice sur les forêts de l'Algérie: Leur étendue, leurs essences, leurs produits* (Algiers, 1878); Henri Lefebvre, *Les forêts de l'Algérie* (Algiers-Mustapha, 1900); Henri Marc, *Notes sur les forêts de l'Algérie* (Algiers, 1916); and Marc, *Notes sur les forêts de l'Algérie* (new ed., Paris, 1930).

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MAP 2: Forested Region III (Constantine and Aurès). Map drawn by Bill Nelson.

and property controlled by the Ottoman government, as well as unused land. These forms of land tenure, as well as traditional land-management practices in forested and agricultural areas, were poorly understood and little respected by the French. One of the most widely used practices in both agricultural and forested areas was *keçir*, a technique that consisted of clearing land for planting by setting controlled fires every four years, a cycle that would allow for the planting and harvesting of crops as well as fertilization from the ashes that were produced. In the intervening years, the forest was left for the grazing of animals.[6]

From the period of the French conquest in 1830, forested lands were used and preserved for military purposes. The law of June 16, 1851, made the forests of Algeria the property of the state. Certain rights were proscribed to their indigenous inhabitants, and most of this land was used for pasturage and the collection of wood. In 1846, however, the French government had started to grant long-term leases or concessions, ceding large tracts of forests to wealthy French businessmen for specific periods of time, which ranged from forty to sixty years. These tracts, which often consisted of lucrative cork trees and Aleppo pine, were then exploited for commercial purposes. Algeria quickly became one of the leading exporters of cork in the world, and Algerian wood was showcased at colonial exhibitions in metropolitan

[6] Charles-Robert Ageron, *Les algériens musulmans et la France (1871–1919)*, 2 vols. (Paris, 1968), 1: 103–106; David Prochaska, "Fire on the Mountain: Resisting Colonialism in Algeria," in Donald Crummey, ed., *Banditry, Rebellion, and Social Protest in Africa* (London, 1986), 230–235; Davis, *Resurrecting the Granary of Rome*, 28–32. Paul Marès remarked in a publication associated with the universal exhibition in Paris in 1878 that "the natives have still kept, under our domination . . . the disastrous custom of setting fires." Marès, *Histoire des progrès de l'agriculture en Algérie* (Algiers, 1878), 13.

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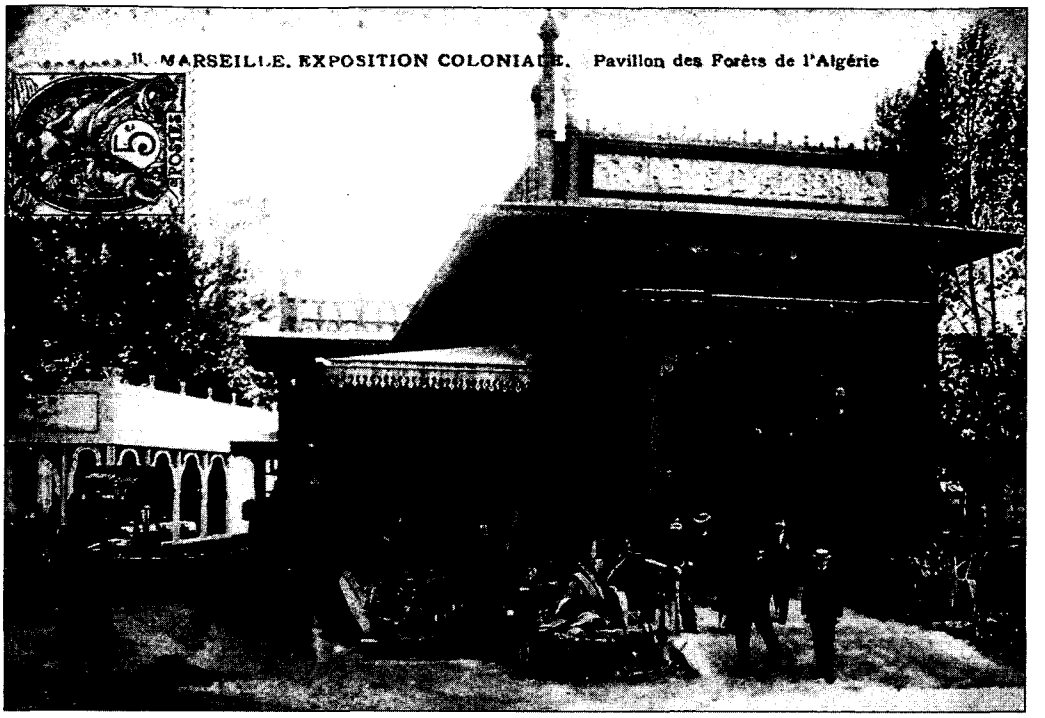


FIGURE 1: Algerian Forest Pavilion at the 1906 Marseille Colonial Exhibition. Research Library, The Getty Research Institute, Los Angeles, California (970031).

France. As a result, *concessionaires* expropriated vast tracts of forested land and barred their use by the indigenous population.

The French applied the 1827 metropolitan forest code to all forested land administered by the French state in 1838, when the Algerian forest service was created, without any modification despite the real differences that existed in terms of the nature of the agricultural economy and forested land in metropolitan France and colonial Algeria. It did not take into account the existing forest economy, which relied both on livestock grazing and on periodic brush fires to provide clearings for the planting of crops. The 1827 law in many respects assumed the existence of uninhabited forests, which was hardly a reality in metropolitan France, and was even less so in Algeria. The forest code of 1827 did not permit the grazing of sheep or goats, but it did allow pigs, whose husbandry would have been largely unimaginable for Muslims. It severely curtailed the cultivation of plots in the forest, forbade the building of *gourbis* (huts) less than one kilometer from a forest, and, as in France, prosecuted those found gathering fallen wood, cork, or other material. The 1827 code was strengthened by the passage of an 1874 law designed to prevent forest fires, which required the indigenous population to fight fires, while imposing collective fines and punishment for setting fires. Although Algeria never experienced a "Guerre des Demoiselles," the protracted nineteenth-century war between forest dwellers dressed as women and the forest administration in metropolitan France, the number of forest violations, and even more significantly the number of forest fires,

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increased steadily in the last quarter of the nineteenth century and continued up to World War I and beyond.[7] (See Table 1.)

In 1900, Algeria possessed 3,247,692 hectares of forested land, 2,503,336 of which were administered by the French forest service (Eaux et Forêts). Only 468,495 hectares belonged to individuals, and much of the most lucrative forested land was exploited by the French.[8] The most prized of the forests ceded to French businessmen were the cork forests (*chêne liège*) in the department of Constantine, from which the indigenous population was slowly expelled. This was an area near the Mediterranean coast, where the vast majority of the French and European populations of Algeria resided, as well as in the Tell, where, according to one observer, "the conditions of existence" were "sensibly the same as in Europe."[9]

French concerns about forest conservation coincided with the emergence of a "declensionist" environmental narrative among the French in Algeria in the 1850s. According to this narrative, forests in North Africa were sites of memory, emblems of North Africa's golden Roman past—which had allegedly been destroyed by the Arab invasions of the seventh and eleventh centuries, when the Maghreb ceased to be the "granary of Rome."[10] According to what must now be acknowledged as a legend, because it was based on inconclusive and in many cases fallacious historical, geographical, and environmental evidence, under Roman domination North Africa had been at once a bountiful and peaceful land until its conquest by the Arabs and later by the Ottoman Empire. These views were based largely on French readings of Herodotus, Strabo, and Tacitus; on archaeological evidence of irrigation networks

[7] The 1827 code was not, however, finally legalized in Algeria until 1883. Prochaska, "Fire on the Mountain," 235. For a history of fire and environmental practices in Algeria, see Stephen J. Pyne, *Vestal Fire: An Environmental History, Told through Fire, of Europe and Europe's Encounter with the World* (Seattle, 1997), 120–125. For the "Guerre des Demoiselles," see Peter Sahlins, *Forest Rites: The War of the Demoiselles in Nineteenth-Century France* (Cambridge, Mass., 1994). An extensive literature was generated during this period on the probable cause of the fires, most of which was published in Algeria. See M. Ch. de Chabannes, *Lettre sur les incendies périodiques des forêts en Algérie adressée à M. Le Baron G. Martineau des Chesnez* (Algiers, 1866); Gouvernement Général de l'Algérie, *Mésures à prendre à l'occasion des incendies des forêts: Rapport presenté à son excellence le gouverneur général de l'Algérie au nom de la commission de Conseil du gouvernement* (Algiers, 1866). J. Reynard, *Restauration des forêts et des paturages du sud de l'Algérie (Province d'Alger)* (Algiers, 1880); Département de Constantine, Conseil Général, session d'octobre 1881, *Incendies des forêts du département de Constantine (août 1881): Rapport de M. Treille* (Constantine, 1881); *Réponse des conseillers généraux indigènes au rapport de M. Treille sur les incendies* (Constantine, 1881); Paulin Trolard, *La cause des forêts de l'Algérie devant la réunion d'études algériennes* (Paris, n.d.); René Rousseau, *Contribution à l'étude de la question forestière an Algérie* (Algiers, 1931).

[8] Rebattu, "Le régime forestier en Algérie," *Bulletin de la réunion des études algériennes*, March–April 1900, 57–63.

[9] Dr. Trolard, *La question forestière en Algérie et le programme de reboisement de gouvernement général* (Algiers, 1885), 442. Trolard simplified the map of Algeria, claiming that there were only three zones—the Tell, the high plateau, and the desert. The Tell was home to sedentary farmers, whereas nomads inhabited the other two zones.

[10] See Davis's extended and thorough discussion of how this narrative came to be constructed in Algeria from its conquest to the twentieth century in *Resurrecting the Granary of Rome*, esp. chaps. 3 and 4. For the place of this Roman heritage in the French imagination, see Patricia M. E. Lorcin, "Rome and France in Africa: Recovering Colonial Algeria's Latin Past," *French Historical Studies* 25, no. 2 (Spring 2002): 295–327. Nabila Oulebsir explores the French initiatives to save the archaeological vestiges of North Africa's Roman past as French *patrimoine* in *Les usages du patrimoine: Monuments, musées et politique coloniale en Algérie (1830–1930)* (Paris, 2004).

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TABLE 1
Number of Forest Fires in Algeria, 1865–1913

Year	*Number*	*Surface hectares*	*Damage in francs*	*Observation*
1865		160,000	2 million	Commission d'enquête
1873		73,313		
1874		2,777		
1875				Insignificant number
1876	120	55,172	441,881	
1877	134	40,538	1,807,061	Numerous Beni-Salah
1878	164	8,156	617,324	
1879	218	17,662	625,987	
1880	137	20,880	353,245	
1881	244	169,056	9,042,000	Commission d'enquête
1882	130	4,018	188,751	
1883	148	2464	102,339	
1884	147	3,231	205,185	
1885	285	51,569	674,487	
1886	288	14,043	270,325	
1887	395	53,714	1,560,920	
1888	311	14,788	176,833	
1889	309	17,807	522,389	
1890	202	23,165	1,786,505	
1891	393	45,924	2,906,459	
1892	409	135,574	6,605,276	
1893	398	47,756	5,303,752	
1894	308	106,889	2,266,043	
1895	250	32,957	324,661	
1896	179	14,091	632,557	
1897	396	79,203	2,468,062	
1898	150	12,384	282,164	
1899	272	16,099	547,766	
1900	162	2,937	143,192	
1901	135	9,687	259,110	Numerous Beni-Salah
1902	475	141,141	3,668,780	Inquiry into 1902–1904 fires
1903	388	94,398	5,329,047	
1904	244	2,759	90,093	
1905	255	7,676	274,084	
1906	219	9,126	399,037	
1907	211	4,457	92,809	
1908	344	6,540	182,339	
1909	278	9,751	653,050	
1910	482	24,294	411,286	
1911	322	16,309	339,148	
1912	338	26,505	377,205	
1913	700	139,625	3,096,958	

SOURCE: "Note sur les incendies de forêts en 1913," CAOM, ALG, GGA, P/128.

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and olive presses; and on Roman coins, which depicted bundles of wheat.[11] The legend was also based on the writings of the fourteenth-century North African historian Ibn Khaldun (1332–1406), whose work was translated into French by Baron de Slane in the 1850s and 1860s.[12] Various authors repeatedly cited Ibn Khaldun's description of Arab nomads as "locusts" who destroyed gardens and trees. Some recent research ironically suggests, however, that the real beginning of soil degradation in North Africa might be better attributed to Roman practices of over-cultivation, and that the greatest amount of deforestation in North Africa took place between 1890 and 1940, when the French virtually decimated Algeria's extensive cork forests for profit—thus a period well after the French had already constructed "their story of the environmental history of North Africa."[13]

Theodore S. Woolsey, a member of the U.S. Forest Service who conducted a comparative study of forests and forestry in Tunisia, Algeria, and Corsica in 1916, argued that there had been a "direct diminution of forest area through the sale of valuable cork-oak lands," noting that "one writer estimates that the forested areas near Constantine, Batna, Médéa, and Sétif have shrunk by from 10 to 60 per cent since 1871."[14] Nonetheless, José Germain and Stéphane Faye, among other contemporary commentators, believed that deforestation had occurred long before, and that it was France's mission to restore the region to its former greatness, while supplying grain and other agricultural produce to the metropole:

> Following the prosperous days of the Roman era, Algeria, Tunisia and Morocco . . . vegetated, barely surviving, cultivating piracy . . . One by one, the former riches disappeared . . . all fell into ruin. Of the splendid golden age of Roman Mauretania only a sun-baked rocky desert remained, where the beneficent river had become a devastating torrent, where agriculture had atrophied, where livestock subsisted and perished according to the vagaries of rainfall, and where the native sought to secure by force or ruse what his neighbor had produced instead of producing it himself.[15]

[11] See Diana K. Davis, "Desert 'Wastes' of the Maghreb: Desertification Narratives in French Colonial Environmental History of North Africa," *Cultural Geographies* 11, no. 4 (2004): 362. A number of historians have debunked the "granary of Rome" myth. See Will D. Swearingen, *Moroccan Mirages: Agrarian Dreams and Deceptions, 1912–1986* (Princeton, N.J., 1987); B. D. Shaw, "Climate, Environment and History: The Case of Roman North Africa," in T. M. L. Wrigley, M. Ingram, and G. Farmer, eds., *Climate and History: Studies in Past Climates and Their Impact on Man* (Cambridge, 1981), 379–403; and G. Barker, "A Tale of Two Deserts: Contrasting Desertification Histories on Rome's Desert Frontiers," *World Archeology* 33, no. 1 (2002): 488–507. See also Diana K. Davis, "Potential Forests: Degradation Narratives, Science, and Environmental Policy in Protectorate Morocco, 1912–56," *Environmental History* 10, no. 2 (2005): 211–238.

[12] Ibn Khaldun, *Les prolégomènes d'Ibn Khaldoun*, trans. W. M. de Slane, 2 vols. (Paris, 1863–1865); Ibn Khaldun, *Histoire des Berbères et des dynasties musulmanes de l'Afrique Septentrionale*, trans. Baron de Slane, 4 vols. (Algiers, 1852–1856). Also see Yves Lacoste, *Ibn Khaldun: The Birth of History and the Past of the Third World* (London, 1984).

[13] Davis, "Desert 'Wastes' of the Maghreb," 370. Some have suggested that massive deforestation occurred worldwide during the nineteenth century, which coincided with the expansion of European empires. Richard P. Tucker and J. F. Richards, eds., *Global Deforestation and the Nineteenth-Century World Economy* (Durham, N.C., 1983).

[14] Woolsey, *French Forests and Forestry*, 50.

[15] Quoted in Swearingen, *Moroccan Mirages*, 29. See André Fribourg, *L'Afrique latine: Maroc, Algérie, Tunisie* (Paris, 1922); José Germain and Stéphane Faye, *Le nouveau monde français: Maroc, Algérie, Tunisie* (Paris, 1924); Stéphane Gsell, *Histoire ancienne de l'Afrique du nord*, vol. 8: *Jules César et l'Afrique: Fin des royaumes indigènes* (Paris, 1928).

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Forests were also frequently seen as sites of resistance on the part of Arabs, and the prevailing judgment in the colony was that the continuing series of forest fires from the 1860s to the First World War had been deliberately set.[16] To some extent, colonists' perception of the *indigène* was little different from that of government administrators and bourgeoisie with respect to the French peasantry at the beginning of the nineteenth century. Conflict concerning the use of forests was similar to what had occurred in metropolitan France, where pasturage and deforestation were said to account for a whole host of ills, including floods from the French Revolution onward.

Davis has argued that the declensionist narrative ultimately served three main purposes: "the appropriation of land and resources; social control (including the provision of labor); and the transformation of subsistence production into commodity production."[17] A closer look at the development of the narrative and the terms in which it came to be articulated, however, suggests that it was a reflection of even larger concerns and anxieties among settlers in Algeria. What was distinctive about calls for forest conservation in colonial Algeria was the way in which commentators linked deforestation to debates about hygiene, race, and climate change, and their relationship to the possible decline of both the French Empire and "civilization" itself.

Louis Alfred Becquerel, a doctor in the faculty of medicine in Paris, wrote in 1864 that one could in general say that "the absence of wood in a country" where it had once been abundant was "the most certain sign of the passage of great conquerors, of an aging civilization or political turmoil."[18] Forests were at the center of a larger struggle to control the African continent, as evidenced by the growing fear of the gradual, and some argued inevitable, encroachments by the Sahara, which would destroy Algeria's Latin, Mediterranean character.

Algeria had a Mediterranean climate in the areas where most French and European settlers lived and farmed, but a vast amount of its territory consisted of desert—indeed, one of the largest deserts in the world, the Sahara. As Ilhem Saida and others have indicated, the French had an ambiguous and complicated relationship to the desert. At times it had a positive, even spiritual or mystical valence, and at others it invoked terror. The two climates of Algeria, those of the shores of the Mediterranean and the vast empty desert of Africa, were at odds with one another, and some viewed the forests as essential to ensuring that the desert would not engulf the whole.[19]

For this reason, debates about reforestation and deforestation in both metropolitan and colonial Algeria centered first and foremost on the question of water. In France, there was a growing consensus that deforestation resulted in too much water, which produced floods; in Algeria, by contrast, the phenomenon was deemed

[16] Prochaska, "Fire on the Mountain," 229–252.

[17] Davis, *Resurrecting the Granary of Rome*, 166.

[18] A. Becquerel, *Traité élémentaire d'hygiène publique et privée*, 3rd ed. (Paris, 1864), 307. He drew on a work by his father, the physicist Antoine-César Becquerel, *Des climats et de l'influence qu'exercent les sols boisés et non boisés* (Paris, 1853), which was also read by George Perkins Marsh for his work *Man and Nature* (1864; repr., Cambridge, Mass., 1965).

[19] See Ilhem Saida, *Mysticisme et désert* (Manouba, 2006); and Jean-Claude Vatin, "Désert construit et inventé, Sahara perdu ou retrouvée: Le jeu des imaginations," in Jean-Robert Henry et al., eds., *Le Maghreb dans l'imaginaire français: La colonie, le désert, l'exil* (Aix-en-Provence, 1985), 107–127.

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to result in too little water, which led to forest fires and climate change in the form of warmer temperatures.[20] Many feared that these temperatures would ultimately hinder colonization by Europeans. One commentator argued that the areas with the most rainfall were forested and that rainfall had been diminishing progressively between 1836 and 1876 in areas that had been deforested. The anxieties implicit in the desiccation narrative surrounding deforestation and reforestation also centered on questions of hygiene, climate, and health, all of which had been debated in the decades following the conquest of Algeria in 1830. Could men and women from colder climates acclimatize and adapt to warmer climates? This question was central to European settler colonialism, and it occupied the British as well. The question of acclimatization was at the heart of debates about colonization.[21]

Becquerel's *Traité élémentaire d'hygiène publique et privée* pointed out that the highest rates of mortality in Algeria in 1844 and 1845 were among Europeans, in contrast to Muslims and Jews.[22] Considerable attention was also given to the impact of the North African climate on women. Achille Fillias, a civil servant who popularized many of the theories that circulated at the time concerning the effect of climate on women's menstrual cycle and reproduction, claimed that pregnant women arriving in Maghreb were more likely to abort there than they would have been in Europe, for example.[23] While he suggested that they had greater difficulty in adapting to a warm climate, Becquerel disagreed, saying that women were in general more likely to acclimatize. Others attributed the insalubrious climate, which led to European mortality, to the lack of rainfall, aggravated by deforestation, contributing to higher temperatures.[24]

THE FIGURE WHO DID PERHAPS MORE THAN ANY OTHER to diffuse these ideas in Algeria at the end of the nineteenth century, when the declensionist narrative was at it height, was Dr. Paulin Trolard. Trolard became the director of the Institut Pasteur of Algers in 1894, a post that he held until he was forcibly removed in 1910—just before his death—for allegedly distributing bad vaccines. He was also a publicist and was

[20] For the impact of forests on moisture and climate during this period, see Caroline Ford, "Nature, Culture and Conservation in France and Her Colonies," *Past & Present* 183, no. 1 (May 2004): 173–198. For the "theory of ruined landscapes" in the Mediterranean, see A. T. Grove and Oliver Rackham, *The Nature of Mediterranean Europe: An Ecological History* (New Haven, Conn., 2001), 8–9.

[21] Alexis de Tocqueville took up the question of acclimatization in his "Second Report on Algeria," written in 1847. In Tocqueville, *Writings on Empire and Slavery*, ed. and trans. Jennifer Pitt (Baltimore, 2001), 176. Trolard reiterated it in his *Les forêts de l'Algérie et la colonisation* (Algiers, 1890), 28. Eric T. Jennings has demonstrated that it was no accident that the French developed a network of colonial spas in the empire, and that Vichy was a central metropolitan place to cure the colonial body exposed to the rigors of climate in France's colonies. Jennings, *Curing the Colonizers: Hydrotherapy, Climatology, and French Colonial Spas* (Durham, N.C., 2006). See also Mark Harrison, *Climates and Constitutions: Health, Race, Environment and British Imperialism in India, 1600–1850* (New Delhi, 1999). The relationship between climate, politics, and human nature was, of course, an old question, and one that Aristotle, Buffon, and Montesquieu addressed. Clarence J. Glacken, *Traces on the Rhodian Shore: Nature and Culture in Western Thought from Ancient Times to the End of the Eighteenth Century* (Berkeley, Calif., 1967). Michael Osborne suggests that acclimatization was "the essential science of colonization." Osborne, *Nature, the Exotic, and the Science of French Colonialism* (Bloomington, Ind., 1994), xiv.

[22] Becquerel, *Traité élémentaire d'hygiène publique et privée*, 307.

[23] Fillias, *Etat actuel de l'Algérie*, 13.

[24] Ibid.; Becquerel, *Traité élementaire d'hygiène publique et privée*, iii.

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elected to the Conseil Général of Algiers. He took up the cause of reforestation in Algeria with a vengeance and was largely responsible for the foundation of the League for the Reforestation of Algeria (Ligue du Reboisement de l'Algérie) in 1882, serving as its president for the duration of its more than twenty-year existence.[25]

The League for the Reforestation of Algeria began with a membership of 1,200 in the three departments of Algeria—Alger, Constantine, and Oran. Its members consisted of French farmers, property holders, and a small minority of Algerians.[26] It supported the study of the problem of reforestation and saw itself as a group that would pressure the French state to act to preserve Algeria's forested landscapes. In an appeal to Algerians in the first issue of the *Bulletin de la Ligue du Reboisement de l'Algérie*, Trolard wrote that "every deforested country is condemned to death!" and that areas in Algeria that were not being destroyed by pasturing were threatened with destruction by the "incendiary Arab."[27] Soon after, a government official who held a post in the Algerian forest service wrote, "it is above all we Europeans who claim to bring modern civilization here" to take charge of landscape conservation. He went on to state, "it belongs to us to repair the ruin caused by fatalism and error" through "the regularization and restoration of the climate of one part of the globe."[28] He also declared at a conference sponsored by the league that history had shown that without maintaining an equilibrium between agricultural and forested land, civilization would soon be destroyed: "These ruins of empires regularly accumulated through the centuries have long been explained by a supposed fatal law which would push the civilization of the Orient into the West . . . It is reserved to our epoch to show the hand of Man destroying . . . the equilibrium between forests and agriculture. In modifying the climate, he changed mores, customs, and destroyed the conditions that had made civilization possible."[29] It was forests that saved the culture of a "civilized" Mediterranean from being engulfed by a barren, uncivilized East and South. Indeed, there were all kinds of predictions that the Sahara and therefore sub-Saharan Africa were creeping toward the shores of the Mediterranean. Trolard warned metropolitan France that the French Empire would experience the same tragedy that befell the Roman Empire in North Africa if it failed to address the problem: "Is it possible to deny that French occupation is coming to an end and very rapidly; and that one can already fix the moment when it will have the same fate as the Roman occupation?"[30] Trolard thus called on France to restore North Africa to

[25] Paulin Trolard, *L'Institut Pasteur d'Alger: Sa fondation, sa réorganisation, mon expulsion* (Algiers, 1910). This work was published posthumously by his sons, who argued that Trolard's unjustified dismissal for allegedly distributing contaminated vaccines hastened his death. Trolard was a prolific publicist and wrote, among other works, *La forêt, conseil aux indigènes* (Algiers, 1883); *La question forestière en Algérie: Les incendies forestiers en Algérie* (Algiers, 1892); *Le testament d'un assimilateur* (Algiers, 1901); and *La politique de la France en Algérie: Aux membres du Parlement* (Algiers, 1897).

[26] Anne Bergeret, "Discours et politiques forestières coloniales en Afrique et Madagascar," *Revue française d'outre-mer* 80, no. 298 (1993): 23–47.

[27] Trolard, "Appel aux Algériens!" *Bulletin de la Ligue du Reboisement de l'Algérie*, January 15, 1882, 2.

[28] M. Reynaud, in *Bulletin de la Ligue du Reboisement de l'Algérie*, May 15, 1882, 4.

[29] Ibid., 91–92.

[30] "Est-il possible de nier que l'occupation française marche à sa fin et à grand pas; et que l'on peut déjà fixer le moment où elle aura le sort de l'occupation romaine?" Trolard, "Appel aux Algériens!" 5. Trolard had his critics. The head of the Société de Climatologie in Alger, Oscar MacCarthy, ques-

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the splendor of its Roman past with the aid of Western science and landscape protection, believing that France would thereby become a new Rome.[31]

Such references to the recovery of the Roman Empire's greatness reflected not only a broader interest in North Africa's Latin past embedded in the "granary of Rome" legend, but also the myth of a new "Latin-Mediterranean race," people destined to reestablish the greatness of North Africa. Discussions of this new, essentially European race became prevalent in a variety of circles in the 1870s and 1880s and came to be intimately linked to environmentalists' concerns about the deforestation of North Africa.

Many found that mixed marriages among the French, Spanish, Italian, and Maltese settler populations were helping to strengthen this supposed "race," and they were heartened by the high birthrates that resulted from these unions, at a time when birthrates were falling in metropolitan France.[32] Onésime Reclus, brother of the great French geographer Elisée Reclus and a frequent traveler to North Africa, made a case for France's abandoning its colonial possessions in Asia in order to concentrate on the African continent, where a "neo-French" race had been born.[33] Louis Bertrand, who was elected to the Académie Française in 1925, did much to foster the importance of a new Latin Africa, and Jules Cambon, who became governor-general of Algeria, praised Bertrand for his interest in its settlers and his "insight into the lives of the Provençaux, the Catalans, the Maltese, and their like, who were at present merging together, as they had done in the time of Rome and Carthage, to form a resistant combination which resonated with the sounds of the Mediterranean."[34] Bertrand also extolled a Christian Africa, epitomized by the great North

tioned Trolard's reading of Roman history, arguing that the Roman Empire did not disappear as the result of droughts but because of political revolutions that forced the Romans out. *Bulletin de la Ligue du Reboisement de l'Algérie*, December 15, 1886, 1186. He argued that Trolard presented Algeria in a "fantastical and exaggerated light" (1182). MacCarthy, a Frenchman of Irish origin who had emigrated to Algeria as the leader of a group of eight hundred colonists in 1848, was a local *savant* and a formidable critic. Sanford H. Bederman, "Oscar MacCarthy, 1815–1894," in T. W. Freeman, ed., *Geographers: Bibliographical Studies*, 26 vols. (London, 1984), 8: 57–60.

[31] *Bulletin de la Ligue du Reboisement de l'Algérie*, December 15, 1886, 1182.

[32] The geographer Onésime Reclus, who coined the term "francophonie" in the 1880s, saw the Italians and Spanish of North Africa as natural allies in the conquest of the African continent in *Lâchons l'Asie, prenons l'Afrique: Où renâitre? et comment durer?* (Paris, 1904), but he curiously anticipated the future mixing of all races: "L'humanité future ne sera pas faite seulement de blonds et de bruns. Qu'on le veuille ou non, tous les peuples se mêleront." Reclus, *L'Atlantide: Pays d'Atlas—Algérie, Maroc, Tunisie* (Paris, 1918), 14. Reclus spent a considerable amount of time in North Africa, and reflected on the problem of colonial expansion in much of his work.

[33] Reclus, *Lâchons l'Asie*, 73.

[34] Quoted in Patricia M. E. Lorcin, *Imperial Identities: Stereotyping, Prejudice and Race in Colonial Algeria* (London, 1999), 198. Bertrand, who first came to Algeria in 1891 to assume a post at the *lycée* of Algiers, visited the Roman ruins of Tipasa in 1895 and declared that he had rediscovered ancestors who spoke his language and believed in his gods, so that he was no longer a "lost Rumi in an Islamic land" (200). *Rumi* was the term used by Arabs and Berbers to designate foreigners, especially Christians. It is derived from the Arabic *rum*, meaning land dominated by Rome. This designation survives among the native population of the Maghreb. In her study of collective memory in Tunisia, the ethnohistorian Jocelyne Dakhlia found that the people of the Jérid spoke of the Romans as the "ancient French" and explained that archaeologists and tourists came to North Africa in search of traces of their ancestors. Dakhlia, *L'oubli de la cité: La mémoire collective à l'épreuve du lignage dans le jérid tunisien* (Paris, 1990), 56–57.

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African saint Augustine, which had been destroyed by the Donatists and by Islam. Indeed, his goal was to return Algeria to its Latin, Christian, and Western origins.[35]

While there was no consensus regarding the virtues of the new "Latin race," and some even argued that only northern Italians had anything in common with the French, the possibility of being engulfed by a vast indigenous population of Muslims who allegedly had nothing in common with Europeans and were in the process of devastating the environment was deemed by many Europeans in Algeria to be a very great peril. The existence of this population made Algeria appear to be radically different from metropolitan France, marking it as a land dominated by Islam, barbarous tribes, and pirates. This perception, according to Trolard and many others, posed almost insurmountable obstacles in terms of encouraging further colonization, without the assurance of a propitious climate, which would be created by reforestation, and therefore an obstacle for the progress of civilization itself.[36]

M. Ollive, who was a member of the League for the Reforestation of Algeria, gave a speech in Sétif in which he argued that long ago, the Ottoman Empire had been rich and populous and had constituted one of the most powerful empires in the world. Then the "Arab race" arrived and brought their practices of setting fires and grazing animals, which led to the disappearance of forests, which resulted in the rivers drying up and climate change.[37] The anti-Arab or anti-Muslim rhetoric became even more pronounced in the pages of the *Bulletin de la Ligue du Reboisement de l'Algérie* ten years later, when it published the findings of a report by a M. Jeannel on the subject of deforestation as a possible cause for depopulation. Jennel argued that deforestation was a cause of sterility, as evidenced in the barren and supposedly uninhabited areas of the steppes of Central Asia, the Sahara, and the Arabian desert.

A series of disastrous periodic fires that began in the 1860s and continued through the First World War initiated a prolonged debate about deforestation and reforestation. French *colons*, the forest service, and the French state increasingly came to see them as a result of arson perpetrated by "Arabs" to resist the French state. They did not take into account the fact that fires regularly broke out in the hot, dry conditions of late summer in North Africa and in other parts of the Mediterranean. Nor did they acknowledge the fire ecology of the region, which entailed controlled burning within forests (*keçir*).[38]

In 1878, following a set of particularly devastating fires, an editorial in the newspaper *Courrier de Bône*, commenting on the extensive damage in the forests of Beni-Saleh in the department of Constantine, postulated that the periodic fires set by the *indigènes* represented the "most serious obstacle that colonization could confront in

[35] Lorcin, *Imperial Identities*, 200; and Louis Bertrand, "Le centenaire du cardinal Lavigerie," in Bertrand, *Devant l'Islam* (Paris, 1926), 83.

[36] "Habitée par les populations indigènes hostiles et quasi réfractaire à toute civilisation européene, l'Algérie présentera toujours une hetérogeneité ethnique qui ne s'accorde guère avec les aspirations de la France à l'égard de sa grande possession africaine." Paoli, "La question des étrangers en Algérie," *Bulletin de la Réunion d'Etudes Algériennes*, nos. 2–3 (February–March 1906): 93. Trolard was skeptical about the new Latin race and preferred to encourage emigration from France, but the issue of climate was all-important for this project. Trolard, *De la mentalité algérienne (à propos de la question des étrangers)* (Blida, 1905), 17. Albert Billiard, "Le péril étranger," *Bulletin de la Réunion d'Etudes Algériennes*, nos. 8–9 (October–November 1905): 270–273.

[37] *Bulletin de la Ligue du Reboisement de l'Algérie*, September 15, 1882, 184.

[38] Prochaska, "Fire on the Mountain," 229–252; Pyne, *Vestal Fire*, 120–125; Grove and Rackham, *The Nature of Mediterranean Europe*, chap. 13.

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Algeria." It went on to assert that "neither the English in India, nor the Americans in the plains of the Far West have encountered as rebellious and hostile an element as this Arab nation animated by Muslim fanaticism and contempt for the riches of the earth."[39] The "clash of civilizations"/anti-Muslim rhetoric and the link made between Islamic fanaticism and environmental devastation came to be articulated in this context and were part of a broader anticlerical sentiment that pervaded Catholic France at the end of the Second Empire and in the early Third Republic. A commission was formed to reflect on the problem of forest fires, composed of French businessmen who had been granted concessions to exploit vast tracts of Algeria's abundant cork forest. Its members were in no doubt about where the trouble lay, writing that "in contrast to the Berber, the Arab is the personal enemy of trees. Everywhere that he establishes himself, he destroys them. This is so evident that on no point of Africa or Asia does one see trees on the territory of an Arab tribe."[40] They went on to claim that the fires that swept the region were caused by ill will "provoked by religious fanaticism," and were even part of a larger Muslim conspiracy against Christianity. The report cited other examples of this conspiracy in the planning of a massacre of Christians at Easter in Larissa in 1856 and in the pillaging of the Christian district of Galata in Istanbul in 1859.[41] This rhetoric, however, became even more pronounced in the pages of the *Bulletin de la Ligue du Reboisement de l'Algérie* by the 1890s. In 1892, for example, the bulletin published the findings of a report in which the author wrote: "Muslims have covered Europe with these ruins. The grass no longer grows where their horses have passed. North Africa, Greece and the largest part of Spain have seen their fertility disappear with the vegetation that covered them."[42]

The power of the myth of Muslim depredations and the "bedouinization" of North Africa can be adduced in the historical narrative surrounding indigenous groups in the region, notably the Kabyles—a non-Arab (Berber) population alleged to have been native to Algeria at the time of the Arab invasions of the seventh and eleventh centuries—and the nomadic population, who were the supposed descendants of the Arab invaders. Abbé Raynal's *L'Histoire philosophique et politique des établissements et du commerce des européens dans l'Afrique septentrionale*, which was published posthumously in Paris in 1826, propagated the "Kabyle Myth." His account was particularly popular from the 1850s on. The largely sedentary and farming Kabyle people, according to his narrative, had been pushed into the forested mountain regions of the Maghreb, while the rich lowlands became denuded and inhabited by nomadic Arab pastoral groups. The Kabyle Berbers were said to be an independent people with a wild taste for liberty, and were the descendants of the Vandals, a Nordic population from across the Mediterranean. They were reputed to be handsome, originally with blond hair and blue eyes, and only incompletely converted to

[39] *Courrier de Bône*, August 29, 1878, 3.

[40] Centre des archives d'outre-mer, Aix-en-Provence [hereafter CAOM], ALG GGA P/128, *Incendies en Algérie, années 1860, 1863, et 1865: Rapport de la Commission d'enquête nommée par l'Assemblée générale des concessionnaires de forêts de Chênes-Liège. Adoption du rapport et de ses conclusions* (Paris, 1866), 29.

[41] Ibid., 34.

[42] *Bulletin de la Ligue du Reboisement de l'Algérie*, January 15, 1892, 1920.

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Islam.[43] During the course of the nineteenth century, French ethnographic texts, administrative reports, and travel literature helped to disseminate the Kabyle Myth and the belief that these mountainous people were superior to and fundamentally different from the lowland Muslim Arabs because of their fierce independence, egalitarianism, respect for property, and sedentary life.[44] The story was largely accepted by French geographers in the interwar period and underpins the work of the great *Annaliste* historian Fernand Braudel, who in *The Mediterranean and the Mediterranean World in the Age of Philip II* wrote: "to the east and to the south the Mediterranean suffered two invasions, in fact two series of prolonged upheavals that altered everything. These were the two 'gaping wounds' spoken of by Xavier de Planhol: The Arab invasions which began in the seventh century and the Turkish invasions which began in the eleventh," which introduced the culture of nomadism into the region.[45] According to Braudel, the "Arab invader" avoided mountainous regions and "skirted the edge of the deserted mountains bordering the sea, where an abundant rainfall accounts for thick forests of antiquity long respected by man."[46] Repeating the Kabyle Myth, Braudel asserted that the "forests, therefore served as refuges for the native populations before the Arab conquerors," and "'Bedouinization' following the Arab conquest spread all over the land in between these mountains, whether early or lately settled, like a flood cutting off the mountain tops as islands."[47] The power of this narrative can also be seen in its pervasiveness, even in the English-speaking world. In 1915, for example, J. Russell Smith, a professor at the University of Pennsylvania, wrote in the *Bulletin of the American Geographical Society* that "Rome once ruled the world—all of it that was worth taking, but she never took the Bedouin. He fled into his desert pastures and left the farm lands to the conqueror. Then Rome fell, and the Bedouin who had helped bring it about came back into his own, turning the Roman farms into pastures."[48] Smith went on to acknowledge Latin Europe's mission to reclaim Rome's glory in North Africa. Perhaps also thinking of the Italians in Libya, he wrote: "And now Rome and her children, the other Romance countries, are taking another turn and are again driving the

[43] Ageron, *Les algériens musulmans et la France*, 1: 268.

[44] For the development of the Kabyle Myth, see ibid., 267–292; and Lorcin, *Imperial Identities*, 2–6. See also Tocqueville, *Writings on Empire and Slavery*, 171–172.

[45] Fernand Braudel, *The Mediterranean and the Mediterranean World in the Age of Philip II*, trans. Siân Reynolds, 2 vols. (New York, 1976), 1: 95–96. Braudel based his observations on the work of Xavier de Planhol, a geographer and Islamologist who held a post at the Sorbonne. See Planhol, "Caractères généraux de la vie montagnarde dans le Proche-Orient et dans l'Afrique du Nord," *Annales de Géographie*, no. 384 (1962): 113–129; and Planhol, "Nomades et Pasteurs," pts. 1 and 2, *Annales de l'Est* 1 (1961): 291–310, 2 (1962): 295–318. Braudel taught for several years in *lycées* in Algeria as a very young man and began his thesis on the Mediterranean, which Lucien Febvre hoped would focus on "La Méditerranée des Barbaresques," but which was later to become *The Mediterranean and the Mediterranean World in the Age of Philip II*. Braudel's experiences and interest in Algeria in his youth have hardly been explored. See Oulebsir, *Les usages du patrimoine*, 325.

[46] Braudel, *The Mediterranean and the Mediterranean World*, 96–97.

[47] Ibid., 97.

[48] J. Russell Smith, "The Desert's Edge," *Bulletin of the American Geographical Society* 47, no. 11 (1915): 813. This narrative—without the purple prose—persisted into the 1940s and 1950s in the North American academic community. See, for example, A. N. Sherwin-White, "Geographical Factors in Roman Algeria," *Journal of Roman Studies* 34, pts. 1 and 2 (1944): 1–10; and Rhoads Murphey, "The Decline of North Africa since the Roman Occupation: Climatic or Human?" *Annals of the Association of American Geographers* 41, no. 2 (1951): 116–132.

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Bedouin back from the Mediterranean shores."[49] Indeed, the drive to stem the tide of deforestation was part of a larger attempt to claim Algeria's landscape as one that was Mediterranean. This is reflected powerfully in literary forms as well, and in the romanticization of the Mediterranean climate. Robert Randau, for example, one of the principal authors in the so-called turn-of-the-century "algérieniste" literary movement, has one of his principal characters exclaim that he would never abandon Algeria for Paris: "No, Paris has wonderful sunsets, and the night there is prodigious in its richness; but my ideal is not at all that of a bookworm . . . I am an adventurer . . . I have a sort of perverse love for tropical countries where nature is massive."[50]

A neglected part of the story of the drive for reforestation in Algeria is the degree to which it was also profoundly shaped by competition between the European powers after 1890. This was not just a question of standing up for "Western Civilization" and Christendom, reclaiming the Mediterranean, or providing a justification for dispossessing an indigenous population and appropriating their land. It was also an extension of the fierce conflicts among the European rivals in the region. M. Ollive argued that reforestation was about the survival of France as a great power. For him, it was a question of whether the "French race" would be able to make further inroads into Africa, which was the challenge that destiny had presented to "yesterday the Great Nation, and perhaps tomorrow the protégés of the Slavs and the Saxons."[51] He declared to his audience:

> you who have felt the tears of rage and of shame well up in your eyes, at the memory of the errors and crimes of those who, presiding over the destiny of our country, led us to Rosbach, to Waterloo, to Sédan, [who] made us lose America, the Indias and the scepter of the world. Will you hesitate when fortune shines [on us] again, permits us to repair the disasters of the past, no longer with the sword, but with the spade, no longer in spilling blood, but in working, protecting the land, a continent, which can again become the most beautiful in the world, the jewel [in the crown], the future of our race?[52]

In an article published three years later entitled "A Colony in Danger," Trolard asked, "what will become of France when the Anglo-Saxon empire has bases in America and Australia, when in the Old World, pan-Slavism and pan-Germanism will have fulfilled their program?"[53] In Trolard's view, France should encourage more French to settle in Algeria, because the disappearance of Algeria would signal the decline of France itself.[54] In order to appeal to potential settlers, the state would have to make a concerted effort to entice them, because the climate did not "suit the very large majority of French temperaments because of its dryness and its proximity to the Sahara."[55] Reforestation through scientific means would increase rainfall

[49] Smith, "The Desert's Edge," 813.

[50] Robert Randau, *Les algérienistes: Roman de la patrie algérienne* (Paris, 1911), 310.

[51] *Bulletin de la Ligue du Reboisement de l'Algérie*, November 15, 1882, 204.

[52] Ibid. The Société d'Acclimatation of Algeria gave Ollive a first class medal for his speech on reforestation. *Bulletin de la Société du Reboisement de l'Algérie*, September 15, 1883.

[53] Trolard, "A Colony in Danger," *Bulletin de la Ligue du Reboisement de l'Algérie*, November 15, 1886, 1176. Tocqueville, who visited Algeria twice, voiced similar views as early as 1841. See his "Travail sur l'Algérie," in Alexis de Tocqueville, *Sur l'Algérie* (Paris, 2003).

[54] "L'Algérie sera colonisée entièrement, ou elle ne sera pas. L'Algérie doit etre peuplée presque entièrement de français, ou elle ne sera pas française." P. Trolard, *La colonisation et la question forestière* (Algiers, 1891), 15.

[55] Ibid., 18, 19.

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and reverse the effects of the North African climate. Trolard therefore called for the reforestation of no less than four million hectares of land in Algeria and suggested that once this task was accomplished, France could begin to take true possession of Central Africa, which was the ambition of all modern nations.[56] In short, while the environmental discourse linking reforestation, colonization, and civilization justified territorial possession by settlers and colonial rule over the "uncivilized" Arab nomad, it also came to be voiced in terms of a desire to preserve Algeria as a "French" and Mediterranean colony whose climate would foster a new Latin race.[57]

MUCH OF THE DRIVE BEHIND REFORESTATION IN ALGERIA came from the European settler population, who frequently clashed with the colonial administration and with Paris, as the career of Dr. Trolard suggests. Indeed, Trolard complained bitterly about the way in which the government in metropolitan France downplayed Algeria's significance for the French Empire and slighted French settlers there—a not infrequent *pied noir* refrain. In 1886, he argued that the metropole ignored the importance of Algeria, which was a part of France, in favor of its possessions in Indochina and the Far East: "The sacrifices that it [the nation] has imposed on itself to save the prestige of the flag in Tonkin, and perhaps to establish a few insignificant trading posts there, will it refuse them [for Algeria] . . . to pluck from ruin and death the two hundred thousand of its children whom it brought to Algeria?"[58]

The tensions between the European settler population in Algeria and the metropole ran high, and were played out in political debates regarding Algeria's governance generally, and environmental policy in particular, from the 1890s on. In 1893, a senatorial commission headed by Jules Ferry was charged with assessing government policies in Algeria.[59] Seven senators, including Ferry, left France and spent more than six weeks in Algeria (from April 19 to June 4, 1892). They traveled four thousand kilometers to gather testimony from both *pied noir* settlers and the Muslim population, and deliberately avoided contact with Algeria's political elite. Forest policy and its reform were central to the commission's agenda, and their report called for relaxing the harsh penalties imposed on a native population for pasturing animals in forested areas, arguing that pastoralism was the basis of their livelihood. Ferry's commission went so far as to propose the creation of a new forest code that would be applicable to the specific conditions in North Africa, but Algeria had to wait almost a decade before this code was enacted in 1903.

After hearing a unanimous call from the indigenous population for tempering the excesses of overzealous forest officials, Ferry issued a blistering attack on the service for prosecuting Algerians for forest violations. He deplored their actions, arguing that whereas forest officials considered the Muslim population to be in a constant state of delinquency, it appeared to be facing destitution. Trolard answered with a scathing critique of the commission's report, arguing that the Arabs themselves would die out after the French had disappeared: "With your own hands, senators,"

[56] Ibid., 80.
[57] *Bulletin de la Ligue du Reboisement de l'Algérie*, January 15, 1892, 1916.
[58] Ibid., November 15, 1886, 1172–1173.
[59] Ageron, *Les algériens musulmans et la France*, 1: 447–477.

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he declared, "you will have dug their grave" through indulgence and solicitude for the nomadic population.[60] The Conseil Général of Algiers (of which Trolard was a member) published a protest against the commission's report, whose pretext, in their view, was "protection of the natives against the settlers," but whose aim was "the destruction of our departmental and communal liberties."[61]

The governor-general of Algeria, Jules Cambon, was sympathetic to Ferry's recommendations, however. In 1893, he noted to the administrator of the mixed commune of Boghari that the Algerians had been fined more than 100,000 francs by a civil court in Blida for grazing their livestock. "These populations are in a state of alarming misery," he observed, and they might end up abandoning the region to avoid forest officials.[62] Six months later, Cambon wrote to the minister of agriculture in Paris and argued that the complaints of the indigenous population in Boghari were "well founded and worthy of every solicitude" from the government, which should remember how the "preexisting rights of these populations have been reduced and distorted little by little by the progressive application of our metropolitan laws."[63]

In May 1895, a report from the commune of Aumale to the governor-general of Algeria indicated that the disputes over forests had as their source "the antinomy that exists in the matter of property between customary or Muslim law and our French law."[64] Numerous conflicts emerged, including one involving a forest fire near Batna in the department of Constantine in 1908 in which the indigenous populations in the El-Ksour stood accused. They responded in Arabic that the village was more than three kilometers from the fire, not close by, and that the sheikh of the *douar* (village) had instructed them to fight the fire. They concluded, "we respectfully point out that the indigenous population of El-Ksour has no interest in seeing the forest burn."[65]

Some government administrators on the ground also showed a degree of sympathy for the plight of the indigenous population. In 1895, the administrator of the commune of Ain Bessem in the department of Alger wrote to the prefect that one could not deny that "our natives show proof of submission and real patience in the presence of the daily vexations to which they are subjected by the subaltern agents of the forest service."[66] He further observed that these "often brutal personnel," ignorant of local mores and the native language, which they did not attempt to learn, "only see in the native a dangerous neighbor," to be taxed and pressed into forced labor at will.[67] He concluded that "the frequency and importance of fires" was directly related to the increase in forest service personnel, who did more to create conditions for insurgency than to plant trees.[68]

In 1892, Cambon made 227,000 hectares of forested land available for livestock herds that were threatened by drought, but he received bad press when exceedingly dry conditions resulted in fires that destroyed 135,000 hectares of forested land in

[60] Dr. Trolard, *La question forestière algérienne devant le Sénat* (Algiers, 1893), 57.
[61] Quoted in Ageron, *Les algériens musulmans et la France*, 1: 456.
[62] Letter, January 11, 1893, CAOM, ALG, GGA P/89.
[63] Letter, June 13, 1893, ibid.
[64] CAOM, ALG, GGA P/89.
[65] Letter, September 2, 1908, CAOM, ALG, GGA P/91.
[66] Letter, February 26, 1895, ibid., P/89.
[67] Ibid. He also noted that one forest official himself kept thirty-five goats on forested land.
[68] Ibid.

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the same year and 101,000 hectares in 1894.[69] Despite his attempts, and those of other government administrators, to rein in the forest service, the situation of the indigenous population probably worsened during these years, judging by the number of petitions submitted to various French ministries and to the president of the republic. They became more numerous between 1895 and 1900, as did the size of monetary fines and *procès-verbaux*, the latter increasing from 12,388 in 1893 to 17,181 in 1898.[70] Moreover, the government appeared unable to stop what was an illegal *de facto* conversion of fines into days of forced labor by the forest service. In the words of one commentator, the service "has never wanted to persuade itself . . . that it was not in France, but in Algeria and in an Arab country."[71]

Algeria had to wait until 1900 and the appointment of a forest commission by the governor-general before recommendations led to the passage of a forest code in 1903 that was specific to conditions in the region. Theodore Woolsey of the U.S. Forest Service suggested in 1916 that many of the difficulties that the French encountered in Algeria were "due to the fact that the Forest Service at first attempted to copy too closely for local necessities, methods used in France."[72] In his address to the commission, Governor-General Charles Jonnart said that he knew of regions where the indigenous inhabitants were sandwiched between lands opened for settlement and forests, but he was satisfied that the 1903 code was more "supple" and sensitive to the needs of the local population.[73] In reality, it retained many of the same punitive aspects of the 1874 fire law with regard to the indigenous population.

The civil administration in Algeria sent mixed signals to both settlers and Algerians. Jonnart, who was largely responsible for introducing the 1903 forest code, to which Trolard objected, joined the Ligue du Reboisement de l'Algérie in 1905 and presided over Algeria's first Arbor Day celebration, where he declared that, like the members of the league, he was "absolutely convinced of the capital importance of reforestation, to which the future" of the colony was linked: "It is there, for French North Africa, a question of life or death."[74]

Conflicts and fires persisted, moreover. In 1903, seventy-three Algerians in Nemour sent a petition to the governor-general to protest a reforestation project that would deny them the use of the land in Bassin de l'Oued Touent.[75] At first, both the governor-general and the prefect wrote that the concerns of the Arab population should be taken into consideration, and the prefect of Oran even suggested that the project be abandoned in view of the protests.[76] Ultimately, however, the governor-general, on the advice of officials in the forest service and the mayor of Nemour, firmly instructed the prefect to enforce the forest code.[77] In 1911, the *conseillers municipaux* of Nemour again petitioned the governor, this time in the name of the

[69] Ageron, *Les algériens musulmans et la France*, 1: 489–490.
[70] Ibid., 491–493.
[71] Rebattu, "Le régime forestier en Algérie," 59.
[72] Woolsey, *French Forests and Forestry*, 50.
[73] Ibid., 51–52.
[74] *Bulletin de la Ligue du Reboisement de l'Algérie*, November 30, 1905, 111.
[75] February 20, 1903, CAOM, ALG, GGA 5 O 14. Protests over the project began in 1896, and the matter was not closed until 1911.
[76] Letter, Subprefect of Oran to Prefect, April 30, 1904, and Letter, Governor-General to Prefect of Oran, July 2, 1904, CAOM, ALG, GGA, 5 O 14.
[77] Letter, Governor-General to Prefect of Oran, October 10, 1906, ibid.

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villages of Sidi-Omar and Ouled-Ziri, arguing that the forest service had unlawfully seized seven hundred hectares from the villagers with no consideration of the fact that they had rights over the land where their ancestors had lived for centuries, and that they had thereby been reduced to ruin and destitution.[78] The governor-general closed the case in a letter to the forest service and to the prefect of Oran, in which he instructed them to inform the petitioners that "it is not possible to give them satisfaction" and that the petition was rejected.[79]

THE ALGERIAN CASE SUGGESTS THAT IN ORDER TO UNDERSTAND the motivations behind landscape protection in different geographical contexts, one must take into account not only the ways in which landscapes become sites of memory or emblems of the past, but also how and why some memories are privileged or erased. It also reminds us that landscapes are "imprinted with our tenacious, inescapable obsessions."[80] While there was no colonial consensus regarding how to manage Algeria's forests and establish a place for the *indigène* in his or her own landscape, forest management and conservation were nonetheless governed by the desire to create an environment that would be suitable and sustainable for settler colonialism, which was less of a concern in other parts of the French Empire.

While the impetus behind landscape preservation was inspired to some extent by scientific thought in metropolitan France, it was taken up and largely propagated by the settler *pied noir* population in Algeria in novel and distinct ways. It was, perhaps not surprisingly, a settler cause, as opposed to an initiative generated in the metropole, and the "environmentalism" espoused by settlers justified territorial displacement, as Diana Davis has so eloquently argued.[81] However, the narrative of environmental degradation was not shaped simply by the attempt to expropriate land for the purpose of transforming "subsistence production into commodity production," because not all land in colonial Algeria was expropriated for this purpose. Following World War I, for example, Governor-General Charles Luytaud, responding to pleas first voiced in 1912, decreed on February 17, 1921, that a series of "national parks" be created mainly in forested areas in the interest of preserving "the natural beauty of the colonies and developing tourism."[82] Article 1 of the decree declared that "forests or parts of forests whose botanical composition, picturesque beauty, or climatic conditions" made them particularly amenable would be set aside as "centers of [both] scientific study and tourism."[83] All hunting and wood gathering by indigenous populations would be prohibited, while the government, in accordance with article 8, would favor the construction of hotels and other forms of accommodation. The colonial government thus established thirteen "national parks" in the depart-

[78] Petition, October 15, 1911, ibid.

[79] Letter, Governor-General to Direction des Forêts and Prefect of Oran, ibid.

[80] Simon Schama, *Landscape and Memory* (New York, 1995), 18.

[81] Davis, *Resurrecting the Granary of Rome*. William Cronon makes a similar point with respect to the creation of national parks and wilderness areas in the United States. Cronon, "The Trouble with Wilderness; or, Getting Back to the Wrong Nature," in Cronon, ed., *Uncommon Ground: Rethinking the Human Place in Nature* (New York, 1996), 79.

[82] P. de Peyerimhoff, "Les 'parcs nationaux' de l'Algerie," in André Aubreville et al., *Contribution à l'étude des réserves naturelles et des parcs nationaux* (Paris, 1937), 132.

[83] Ibid.

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ments of Alger and Constantine between 1923 and 1927.[84] These parks, from which all human settlement and land use would be prohibited, represented an attempt to conserve the ancient landscapes of Mediterranean North Africa and to serve as a buffer zone in the face of encroachments by the Sahara.

An overriding sentiment of anxiety and potential loss pervaded environmental narratives regarding deforestation and landscape conservation in colonial Algeria. The environmentalism espoused in Algeria was inspired by settler anxieties regarding the perceived limits of further colonization and territorial expansion in the face of competition with other European empires, an intractable climate, the Saharan desert, a far more numerous indigenous Muslim population, and the fate of Western "civilization" itself. This environmentalism also reflected a desire to claim Algeria as a Mediterranean landscape, which was linked to a Christian Europe.

John H. Elliott has argued in the context of the Atlantic world that "in one of the paradoxes that runs through the whole history of metropolitan-colonial relationships," colonists, "even while coming to appreciate the qualities that made their environment unique, devoted a great deal of time and energy to making it resemble as closely as possible the environment that they had left behind."[85] This was no less true of Algeria at the end of the nineteenth century, where French *colons* allegedly attempted to recover a bountiful Mediterranean landscape and to complete the process of colonization. Like the Europeans of colonial New England, the *pied noirs* of Algeria sought to effect "changes in the land," which led to a restructuring of the nature of land use as well as the organization of plant and animal communities.[86] That European settlers were searching for the familiar in the unfamiliar was apparent in their attempt to create a "neo-Europe."[87] In contrast to North America and other French or British colonial possessions, however, they ultimately viewed these changes as a form of historical "restoration."

Thus the environmental consciousness that emerged among European settlers in North Africa did not constitute a utopian yearning of the kind that Richard Grove identifies in the context of tropical Edens such as Mauritius in the seventeenth and eighteenth centuries, where Europeans searched for the "normative location for social Utopias and the simultaneous formulation of an environmental critique."[88] The "nascent environmentalism" of Bernardin de Saint-Père on Mauritius, and later Edward Balfour in India, Ernst Dieffenback in New Zealand, and Paul Strzelecki in Australia, inspired as it was by the "social reformism of physiocracy and the Enlightenment," had little to do with the "violence of environmentalism" that was in-

[84] For colonial Algeria's "national parks," see Gouvernement Général de l'Algérie, Service des Eaux et Forêts, Commissariat Général du Centenaire, *Les parcs nationaux en Algérie* (Algiers, 1930).

[85] John H. Elliott, "Introduction: Colonial Identity in the Atlantic World," in Nicholas Canny and Anthony Pagden, eds., *Colonial Identity in the Atlantic World, 1500–1800* (Princeton, N.J., 1987), 9–10. William Cronon makes a similar point and highlights the divergent perceptions of landscape among settlers in North America and Native Americans in the seventeenth century. Cronon, *Changes in the Land: Indians, Colonists and the Ecology of New England* (New York, 1983).

[86] Cronon, *Changes in the Land*, xv.

[87] Jennings has shown how the French sought out landscapes that resembled those in France as sites for colonial spas. Antsirabe, Madagascar's spa retreat, was admired for its "Frenchness" because of its physical resemblance to the Massif Central, for example. Jennings, *Curing the Colonizers*, 141.

[88] Grove, *Green Imperialism*, 481.

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herent in the Ligue du Reboisement de l'Algérie in 1900.[89] Since its conquest, Algeria had long been regarded as having a climate that was impossible for Europeans, one that required profound alteration by wresting the land from the Muslim Arabs who allegedly had defiled it and who were incapable of responsible stewardship. The struggle for the environment was cast in terms of a "clash of civilizations" and hygiene. In 1862, Fillias wrote that it was thought that "the climate of Algeria is the most murderous on the globe," and the number of immigrants was diminishing. He argued that this was because Arabs had left "detritus of all sorts" to accumulate for centuries, which created unhealthy miasmas for the soldier and settler. He suggested that little by little, the soil had actually been purified, and sanitary conditions were improving: "Today, thanks to the work undertaken or being undertaken, the climate of Algeria is truly [and] exceptionally healthy."[90]

Myths regarding natural landscapes appear to be an inherent part of their mastery, as well as of the profound anxieties surrounding their potential loss, as recent work on the conquest and Russification of the Central Asian steppes suggests.[91] The Algerian case reflected all-too-familiar processes of expropriation that occurred in other parts of the world, from Central Asia to North America and Australia. Unique to Algeria, however, were not only some of the myths that were invoked, but also the anxieties that the settler population expressed regarding the threat of competing empires, climate, race, and the progress of "civilization" itself at the end of the nineteenth century. This suggests that there was a diversity of colonial environmental projects, shaped by differing forms of colonial rule at different historical moments in time.[92] In short, the Algerian case reveals some of the darker sides of environmentalism in a context of colonial domination and indicates that there was no linear, reformist, or enlightened historical path for environmentalism from the past into the future.

[89] Ibid. I borrow the term "violence of environmentalism" from Ajay Skaria, who explores the conflicts between the British and Dangis in western India. Skaria, *Hybrid Histories*, 192.

[90] Fillias, *Etat actuel de l'Algérie*, 10. He also said that the climate of Algeria was analogous to that of the Midi in France (1).

[91] Willard Sunderland, *Taming the Wild Field: Colonization and Empire on the Russian Steppe* (Ithaca, N.Y., 2004).

[92] For "pluralized colonialisms," see Thomas, *Colonialism's Culture*, and Ann Laura Stoler, "Rethinking Colonial Categories," 135.

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Review Essay

What's Left? Popular Political Participation in Postwar Europe

BELINDA DAVIS

SINCE THE LATE 1990S, THERE HAS BEEN A PROLIFERATION of historical attention to popular politics in Europe in the postwar era. The first gush of a flood of research in progress, these publications offer new insights not only concerning contemporary Europe, but also regarding the nature of politics and political change more broadly, and pertaining to the historian's practice as well. Collectively, this body of work advances an alternative political history of postwar and Cold War Europe, demonstrating that there were regular and connected manifestations of political expression all across the continent and beyond, occurring outside formal channels and involving broad populations. In offering a fresh look at the sources of transformation in late-twentieth-century European political cultures, and at how many Europeans imagined and in turn produced "European values," such works generally demand that longer-term processes and singular events be understood through broad human experience, perception, communications, memory, and agency. Read together, these studies offer significant perspectives on the nature of politics, and of political change, useful beyond the subject of contemporary Europe. Indeed, although it is most often implicit in the work, this historiography poses challenges to the utility of basic modernist political terminology. It reveals at the same time how difficult it can be to write outside these conventional frameworks and attendant vocabularies, or to work through the implications of terms such as "revolution" and the notion of a political "left" and "right." Finally, many among this first wave of historians of contemporary popular politics in Europe write both as critical scholars and as invested contemporaries. Their works demonstrate the rewards of such efforts, and also expose impediments to this task. Altogether, through their shortcomings as well as their considerable achievements, the ambitious early studies of this historical literature provide wide-ranging and important lessons.

The year 1945 brought the military defeat of Nazism and fascism in Europe; 1953 saw the death of Joseph Stalin, whose methods were then denounced by Nikita Khrushchev three years later. In this same period, the developing Cold War divided the continent, not least between two models of politics and governance. How satisfactorily do such events and processes explain the materialization of particular

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political cultures across the emerging entity of postwar Europe?[1] Does an understanding of the effects of American culture—which reached across the continent—complete the picture? The historiography of popular politics, broadly defined, offers fresh insight into these political cultures and their formation and transformation. Scholars and others have long acknowledged the significance of informal politics in Europe after 1945; these politics mark key dates in popular memory, such as 1956, 1968, and 1989. But as historians, the scholars considered here offer both a longer view of these politics and broader context than others have invoked. At their best, they afford nuanced and critical alternatives to the myths that these dates have generated. At the same time, in contrast to other contemporary historians, many of these scholars insist on the importance and the positive nature of this type of popular activity, finding it to be a source for the creation or continual re-creation of "democracy."

Five studies serve here as examples of this recent literature.[2] They are highly varied, and in two cases they transcend the chronological or geographical boundaries of postwar Europe. But each contributes to our comprehension of that time and place, and to our understanding of political change, through attention to popular politics. Offering remarkable spatial and chronological perspective, Geoff Eley's *Forging Democracy: The History of the Left in Europe, 1850–2000* makes the case that broad, shifting populations, identifiable as a coherent political left, have advanced democracy in Europe, if not exclusively, then with relative consistency and efficacy.[3] In the postwar period, Eley sees informal political actors as among the leading exponents of this pattern. A view across time and especially space, tracing personal and political connections among diverse populations, characterizes Padraic Kenney's *A Carnival of Revolution: Central Europe, 1989*. Focusing exclusively on the very recent past, Kenney finds the sources of the 1989 watershed in numerous, often mutually influential informal political groupings that developed in the preceding years, well

[1] "Postwar Europe" often refers to the period 1945 to 1989, but the end date is variable, as the work considered here demonstrates. The notion of Cold War Europe should not obscure the continent's political variability throughout the period, including shifting alliances and unallied nations, authoritarian regimes in Western Europe (Spain, Portugal, and Greece), and more and less residually Stalinist Eastern European countries. I use the terms "Western Europe" and "Eastern Europe" because of their contemporary Anglophone dominance, despite the limitations in meaning, and recognizing that many "Eastern Europeans" prefer the characterization "Central European." Likewise, I use the terms "East Germany" and "West Germany," rather than "German Democratic Republic" and "Federal Republic of Germany." These uses may not be simply geographical, but should not be confused here with the overdetermined and problematic terms "West" and "East," whether as Euro-America versus Asia, as "us" versus "them," or as "good" versus "bad." Definitions of Europe varied across the period and population; reference here to work on some countries and not others is not intended as a judgment.

[2] Such historiography, including seminal examples of oral history, first trickled in in the late 1970s, related to the rise of contemporary history, new cohorts of historian-activists, and "anniversaries" of various events. Compare Tilman Fichter and Siegward Lönnendonker, *Kleine Geschichte des SDS: Der sozialistische deutsche Studentenbund von 1946 bis zur Selbstauflösung* (Berlin, 1977); Ronald Fraser et al., *1968: A Student Generation in Revolt* (New York, 1988); Luisa Passerini, *Autobiography of a Generation: Italy, 1968* (Hanover, N.H., 1996; Italian ed., *Autoritratto di gruppo*, 1988); Peppino Ortoleva, *Saggio sui movimenti del 1968 in Europa e in America* (Rome, 1988); David Caute, *The Year of the Barricades: A Journey through 1968* (New York, 1988); Armin Mitter and Stefan Wolle, *Untergang auf Raten: Unbekannte Kapitel der DDR-Geschichte* (Munich, 1993). Cf. Belinda Davis, "The Personal Is Political: Gender, Politics, and Political Activism in Modern German History," in Jean Quataert and Karen Hagemann, eds., *Gendering Modern German History: Rewriting Historiography* (New York, 2007), 107–127.

[3] Geoff Eley, *Forging Democracy: The History of the Left in Europe, 1850–2000* (Oxford, 2002). Eley frankly acknowledges far less attractive pursuits of "the left."

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beyond the better-recognized Solidarity movement in Poland.[4] Lawrence Wittner's magisterial and engaging *Toward Nuclear Abolition: A History of the World Nuclear Disarmament Movement, 1971 to the Present* defies conventional characterizations to trace a consistent and effective postwar peace movement that both anticipated and succeeded the gargantuan demonstrations of the early 1980s.[5] Like Kenney and Eley, Wittner connects episodes of relevant protest transnationally as well as multinationally, in this instance across the globe. His work reminds us that Europeanists do not have a monopoly on the contemporary history of informal politics across time and space.[6] But Europe constitutes a significant case for this literature, in the density of extant and forthcoming historical work, and in the density of the informal political networks these studies reveal, forming a critical element of the postwar European polity. Wittner's study clarifies these informal political connections within Europe as well as between Europeans and those outside the continent. Insofar as such networks were not unique to Europe, their study provides insight into other cases as well.

The breadth represented by these three texts is still uncommon among histories of the modern era, in contrast to the work of social scientists. This attention to recuperating lost connections, among far-flung populations and across years, characterizes the remaining key texts as well, although they are more focused in space and/or time. They share some similarities in rewriting conventional histories of postwar Europe, and in offering a broader view of how politics "works" from the perspective of popular action. Gary Bruce's *Resistance with the People: Repression and Resistance in Eastern Germany, 1945–1955* examines the unrest of June 17, 1953, which is often identified as having been a unique protest event in East Germany until the demonstrations of 1989.[7] Building on recent historiography, Bruce asserts that there were related instances of popular unrest across the country that pre- and postdated that event by weeks and even years.[8] Viewed from this perspective, he argues, this was a revolution, and indeed a successful revolution. Like other historical work since 1989 on the "Eastern Bloc," Bruce demonstrates that this region was not universally populated by masses too repressed to ever express their concerns and desires. Finally, the anthology *Il lungo decennio: L'Italia prima del 68* stretches the "explosion" of 1968 back to 1945, reconstructing processes of political and social transformation through the experience and actions of what were often small, in-

[4] Padraic Kenney, *A Carnival of Revolution: Central Europe, 1989* (Princeton, N.J., 2003).

[5] Lawrence Wittner, *Toward Nuclear Abolition: A History of the World Nuclear Disarmament Movement, 1971 to the Present* (Stanford, Calif., 2003), is the final volume of his trilogy on the international postwar peace movement, *The Struggle against the Bomb*.

[6] Wittner was trained as a historian of the United States. Compare his seminal contemporary history *Rebels against War: The American Peace Movement, 1941–1960* (New York, 1969). Certainly there is a significant and influential growing historiography on contemporary informal politics outside both Europe and the United States.

[7] Gary Bruce, *Resistance with the People: Repression and Resistance in Eastern Germany, 1945–1955* (Lanham, Md., 2003).

[8] Compare Mitter and Wolle, *Untergang auf Raten*; Ilko-Sascha Kowalczuk et al., *Der Tag X, 17. Juni 1953: Die "Innere Staatsgründung" der DDR als Ergebnis der Krise 1952–54* (Berlin, 1995); Ulrike Poppe et al., eds., *Zwischen Selbstbehauptung und Anpassung: Formen des Widerstandes und der Opposition in der DDR* (Berlin, 1995); Henrik Bispinck et al., eds., *Aufstände im Ostblock: Zur Krisengeschichte des realen Sozialismus* (Berlin, 2004). Bruce considers activists within formal parties alongside others: as opposition party members had no formal political voice as such, their activism was largely equivalent to that of informal oppositional protesters.

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formal groups.[9] Postwar republicanism, rising "Europeanism," and other trends informed popular activism, particularly after 1956. But this activism was not just the nugatory or even pernicious by-product of these earlier trends, springing up suddenly in 1968—or in Italy's "hot autumn" of 1969. Practitioners of popular politics also actively transformed Italian political culture over many years.[10] This volume represents one of the fruits of a multi-year collective project. As innumerable conferences, anthologies, group projects, and dissertations in progress demonstrate, popular politics in the 1960s has become above all a new growth field for historians—especially with respect to Western Europe, but also concerning Eastern Europe—and the work being done is often both comparative and transnational.[11]

In the case of the protest in and around 1968, in particular, the general attention is not new. Nor is an understanding of that year's events as significant across national boundaries, or even the sense that this was the culmination of processes that had been under way for a number of years. The value of the recent work covering the longer postwar era is in painstakingly recuperating a broad range of political activity, often less attention-grabbing than, for example, massive demonstrations, and often

[9] Carmelo Adagio et al., eds., *Il lungo decennio: L'Italia prima del 68* (Verona, 1999).

[10] Among historians treating a "long '68," see Luca Baldissara, ed., *Gli anni dell'azione collettiva* (Bologna, 1997); Arthur Marwick, *The Sixties: Cultural Revolution in Britain, France, Italy, and the United States, c. 1958–c. 1974* (New York, 1998); Mario König et al., eds., *Dynamisierung und Umbau: Die Schweiz in den 60er und 70er Jahren* (Zurich, 1998); Axel Schildt et al., eds., *Dynamische Zeiten: Die 60er Jahre in den beiden deutschen Gesellschaften* (Hamburg, 2000); Geneviève Dreyfus-Armand et al., eds., *Les Années 68: Le temps de la contestation* (Brussels, 2001); Bernard Brillant, *Les Clercs de 68* (Paris, 2003); *Die Siebzigerjahre: Gesellschaftliche Entwicklungen in Deutschland*, *Archiv für Sozialgeschichte* 44 (Bonn, 2004); Jeremy Varon, *Bringing the War Home: The Weather Underground, the Red Army Faction, and Revolutionary Violence in the Sixties and Seventies* (Berkeley, Calif., 2004); Francesco Barbagallo et al., eds., *La trasformazione dell'Italia: Sviluppo e squilibri*, vol. 2: *Istituzioni, movimenti, culture* (Turin, 2006); Christina von Hodenberg and Detlef Siegfried, eds., *Wo "1968" liegt: Reform und Revolte in der Geschichte der Bundesrepublik* (Göttingen, 2006); Detlef Siegfried, *Time Is on My Side: Konsum und Politik in der westdeutschen Jugendkultur der 60er Jahr* (Göttingen, 2006); Miguel Gómez Oliver, "El Movimento estudiantil durante el franquismo (1965–1975)," Colloquium Movimento Estudiantil, Coimbra, May 2006. On the long prehistory of 1989, addressing the range of sources, see Gale Stokes, *The Walls Came Tumbling Down: The Collapse of Communism in Eastern Europe* (New York, 1993); also Charles S. Maier, *Dissolution: The Crisis of Communism and the End of East Germany* (Princeton, N.J., 1997); Hartmut Kühn, *Das Jahrzehnt der Solidarność* (Berlin, 1999).

[11] Among the sources of this new historical work, compare projects from centers of contemporary history (frequently interdisciplinary), such as the Società Italiana per lo Studio della Storia Contemporanea and the Zentrum für Zeithistorische Forschung; book series; and comparative undertakings that bring in younger scholars, as at the University of Bielefeld and at the Open University, and the broader European Union–sponsored scholarly venture "European Protest Movements since the Cold War: The Rise of a (Trans)National Civil Society and the Public Sphere after 1945." See among anthologies, here especially transnational and comparative, Etienne François et al., eds., *1968—ein europäisches Jahr?* (Leipzig, 1997); Aldo Agosti et al., eds., *La cultura et i luoghi del '68* (Milan, 1998); Carole Fink et al., eds., *1968: The World Transformed* (Cambridge, 1998); Ingrid Gilcher-Holtey, ed., *1968: Vom Ereignis zum Gegenstand der Geschichtswissenschaft* (Göttingen, 1998); Belinda Davis et al., eds., *Changing the World, Changing Oneself: Political Protest and Transnational Identities in the 1960s/70s West Germany and U.S.* (New York, forthcoming). Among monographs, see Fraser, *1968*; Marwick, *The Sixties*; Ingrid Gilcher-Holtey, *Die 68er Bewegung: Deutschland, Westeuropa, USA* (Munich, 2001); Michael Schmidtke, *Der Aufbruch der jungen Intelligenz: Die 68er-Jahre in der Bundesrepublik und den USA* (Frankfurt, 2003); Thomas Jørgensen, *Transformations and Crises: The Left and the Nation in Denmark and Sweden* (New York, forthcoming); Guya Accornero, "Entre os verdes anos e os anos de chumbo: Lutas estudantis e resposta política em Itália e Portugal—Dois regimes em comparação" (Ph.D. diss., University of Lisbon, forthcoming). Of particular note is emergent work on 1960s activism in Eastern Europe; for comparative and transnational examples, see Martin Klimke and Joachim Scharloth, *1968 in Europe: A Handbook on National Perspectives and Transnational Dimensions of 1960s/70s Protest Movements* (New York, forthcoming); and forthcoming work by Zdeněk Nebřenský and Gyula Virág.

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carried out by unexpected populations. Collectively, these works intimate that in performing "democracy" in particular ways, such acts thereby contributed significantly to creating democracy, across a continent in which the practice had a highly uneven past and present. These scholars offer complementary or alternative narratives to those presented in the rich social science scholarship on postwar European politics, including popular politics. In comparison with the most compelling social science work, such as the literature of "new social movements," some of this new research foregrounds lived experience among individuals and groups (a practice, to be sure, that was borrowed in part from anthropologists and sociologists), involving an especially wide range of actors and activities, often over a long period.[12] Through this means it brings broader trends to bear, such as Cold War cultures, as well as specific events, including the nuclear disaster at Chernobyl that moved many of Padraic Kenney's subjects to action, while maintaining the focus on that action. There is at the same time a sometimes surprising lack of reference to the relevant social science literature and its findings in this scholarship, despite contemporary historians' embrace of interdisciplinarity.

This new work differs from other significant and illuminating political and politico-cultural histories of postwar Europe as well, which can be divided into three general trends. The first focuses primarily on formal politicians, and on other elites, if sometimes in the context of their reaction to popular political pressures.[13] In contrast, the literature under consideration here emphasizes the role and influence of informal political actors, who often created their own "international relations." In addition, this new scholarship examines moments of close if frequently acrimonious cooperation between these actors and professional politicians. The second historiographical trend highlights processes of "liberalization," also characterized variously as "democratization," "Westernization," and "Americanization." As scholars have applied these terms to Western Europe, this work frequently emphasizes top-down influences, whether in reform politics or broader cultural patterns, rather than the more interactive processes emphasized, for example, by the contributors to *Il lungo decennio*. Some of these studies also imply a kind of linear progressiveness. For

[12] Still more attention to experience would be useful in the historical literature. The premier new social movement scholars include Georg Ekiert, Myra Marx Ferree, Bert Klandermans, Doug McAdam, Donatella della Porta, Dieter Rucht, and, seminally, Alain Touraine and Cornelius Castoriadis. Such work is mushrooming in Eastern Europe. There are numerous interdisciplinary European centers devoted to this type of research, many of which include historians. Addressing what is best and least well borrowed from social scientists on this subject, see Simona Urso, "Il lungo decennio nel lungo dopoguerra," in Adagio et al., *Il lungo decennio*, 11–33. Some European scholars claim that there is little meaningful disciplinary distinction.

[13] Compare, e.g., John W. Young, *Cold War Europe, 1945–1989: A Political History* (London, 1996); Robert English et al., *Rebirth: A Political History of Europe since World War II* (Boulder, Colo., 1999); Jan Pauer, *Prag 1968* (Bremen, 1993); Serge Bernstein et al., *Les cultures politiques en France* (Paris, 1999); Erin Mayan, *Kennedy, de Gaulle, and Western Europe* (New York, 2003); Curt Gasteyger, *Europa zwischen Spaltung und Einigung* (Baden-Baden, 2006); also Owen Chadwick, *The Christian Church in the Cold War* (Harmondsworth, 1993); Volker Berghahn, *America and the Intellectual Cold Wars in Europe* (Princeton, N.J., 2002). Compare important work on interaction between governments in power and informal activists: Paul Ginsborg, *A History of Contemporary Italy: Society and Politics, 1943–1988* (New York, 2003); transnationally, Jeremi Suri, *Power and Protest: Global Revolution and the Rise of Détente* (Cambridge, Mass., 2003); Martin Klimke, *The "Other Alliance": Global Protest and Student Unrest in West Germany and the U.S., 1962–72* (Princeton, N.J., forthcoming); also Lawrence S. Wittner, "Peace Movements and Foreign Policy: The Challenge to Diplomatic Historians," *Diplomatic History* 11, no. 4 (1987): 355–370. This division among these literatures represents general trends; there is also important overlap.

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their part, some scholars of Eastern Europe intimate the "artificiality" of Soviet-influenced governments in preventing the presumed natural processes of democratization.[14] In contrast, Eley explicitly claims to reject such models. The historians discussed here generally trace out the ongoing popular political activity that *de facto* asserted the right to such expression and its influence, interacting with changing cultural patterns, formal political policy—and other informal political actors. This work of continual re-creation is a compelling emphasis that emerges despite the use in some of these works of political vocabularies and terms such as "revolution," terms that might communicate conflicting models of politics and political change.

The final general trend, insofar as it can be so constituted, takes a far more dubious view of Europe's postwar balance sheet. Most prominently exemplified by Tony Judt's *Postwar: A History of Europe since 1945*, this work emphasizes the twin dominance of fascism and communism in the European twentieth century, and challenges the thoroughness with which countervailing forces in the postwar era superseded this anti-democratic legacy, including in the light of convenient "amnesias," until at least the 1990s.[15] The scholars of popular politics find grassroots activists' attention to a "positive" past to be salutary, whether it was drawn from individual memory, forms of collective memory, or reconstituted memories; they note, too, how activists across the continent constructed through this means a particular vision of "Europe," even before 1989. Predictably, activists did not start anew each time they acted; rather, they often saw themselves as acting in some tradition, however robust. In turn, they reinforced, strengthened, and legitimated traditions, creating patterns of change. Some of these historians share a concern for what remains a fragile democratic tradition across Europe, and for what a less bright future economically and otherwise might bring. It is precisely in order to highlight these histories of popular activism, and to thereby further encourage such actions, that several of these authors write. Some see in the attention to this past the best chance of maintaining and expanding democracy, which is only as certain as the commitment to actively renew it. Certainly historians have long written with hopes of inspiring readers, including scholars of popular activism; their work has constituted a growth field in the postwar era.[16] This

[14] Variously, Giuseppe Mammarella, *L'Italia contemporanea (1943–1985)* (Bologna, 1985); Walter Laqueur, *Europe in Our Time: A History, 1945–1992* (New York, 1992); Manfred Welti, *Die 1960er Jahre: Versuch einer mentalitätsgeschichtlichen Gesamtschau* (Basel, 1999); especially on West Germany, Anselm Doering-Manteuffel, *Wie westlich sind die Deutschen? Amerikanisierung und Westernisierung im 20. Jahrhundert* (Göttingen, 1999); Ulrich Herbert, ed., *Wandlungsprozesse in Westdeutschland: Belastung, Integration, Liberalisierung 1945–1980* (Göttingen, 2002); Edgar Wolfrum, *Die geglückte Demokratie: Geschichte der Bundesrepublik Deutschland von ihren Anfängen bis zur Gegenwart* (Bonn, 2007). Bruce, *Resistance*, does imply a natural path repressed; compare Gert-Joachim Glaeßner and Klaus Sühl, eds., *Auf dem Weg nach Europa: Europäische Perspektiven nach dem Ende des Kommunismus* (Opladen, 1994). Work on "Americanization," with its complex influences, is very useful. But compare Bernd Weisbrod, "Eine ganz unamerikanische Tante: Die BBC und der nationale Medienstil in der Nachkriegszeit," in Belinda Davis et al., eds., *Alltag—Erfahrung—Eigensinn: Historisch-anthropologische Erkundungen* (Frankfurt, forthcoming).

[15] Tony Judt, *Postwar: A History of Europe since 1945* (New York, 2006); also variously Mark Mazower, *Dark Continent: Europe's Twentieth Century* (New York, 1999); István Deák et al., eds., *The Politics of Retribution in Europe* (Princeton, N.J., 2000). Compare Gabriel Jackson, *Civilization and Barbarity in 20th-Century Europe* (Amherst, N.Y., 1999); broadly, E. J. Hobsbawm, *The Age of Extremes: A History of the World, 1914–1991* (New York, 1996); Martin Gilbert, *A History of the Twentieth Century*, vol. 3: *1952–1999* (New York, 2000).

[16] Compare E. J. Hobsbawm and George Rudé, *Captain Swing* (New York, 1968); also E. P. Thompson, *The Making of the English Working Class* (London, 1963); Howard Zinn, *A People's History of the*

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purpose makes it all the more important to cast a critical eye on this past. The very practice of popular activism may serve to create and reinforce democracy. But are all popular politics then "good"? Were there always so many "resisters"? Should an imagined broad readership take heart from this past, in the context of particular "European values," without close attention to other, less attractive such values, and how these have also gotten re-instantiated?

THE RECENT HISTORIOGRAPHY ON POPULAR POLITICS IN POSTWAR EUROPE offers significant insights, both by expanding the view of "politics" and by examining such politics across time, space, and populations. Collectively, it treats in this context a broad range of forms of political expression and activism, as well as a wide variety of specific goals. While there are still gaps in historical treatment, the current work highlights patterns in a score of European nations. This scope includes attention to popular politics connected across national boundaries, across the divided continent, and with countries beyond, offering fresh perspectives on postwar Europe, Cold War Europe, and Europe in a "globalizing" world. If it is a commonplace that 1968 and 1989 represented transnational as well as multinational episodes of popular political expression (indeed a hallmark of the modern era), these studies begin to flesh out the actual connections among broad groupings and to identify their sources—for example, in a particular postwar mobility, and in expanded if highly uneven access to media and communications. Moreover, while earlier scholars tried to divine the causes for the simultaneity of protest across distances, these historians are finding new answers by examining a longer time period, tracing the influence of one set of protests—and other, often less prominent acts—on the next. They identify the role of collective memory in informing activism, and in turn the cumulative effects of popular political action, on the activists themselves and on their societies, asserting thereby a particular vision of "Europe." Through this means, these studies shed new light on transformations in postwar political culture across Europe, and on the workings of political and social change more broadly.

What, exactly, was this "popular politics"? We might think of public demonstrations, or of the underground literature that Padraic Kenney emphasizes, both longstanding modes of popular political activity.[17] But such forms only scratch the surface of what these historians collectively educe, practices that grew out of imagination, necessity, and a desire to be heard. One might argue that these forms could be as significant as the highly variable demands they advanced, for example, chal-

United States, 1492–Present (New York, 1979); also, e.g., work on themes of "resistance," domestic and otherwise, to fascism and Nazism.

[17] For "self-published" literature from the Enlightenment era, compare Jonathan I. Israel, *Radical Enlightenment: Philosophy and the Making of Modernity, 1650–1750* (New York, 2001); Robert Darnton, *The Literary Underground of the Old Regime* (Cambridge, Mass., 1982); from interwar Russia, Dietrich Beyrau, *Intelligenz und Dissens: Die russischen Bildungsgeschichten in der Sowjetunion 1917–1985* (Göttingen, 1993). Official media in Eastern Europe, and at certain times in Portugal, Spain, and Greece, were highly limited, and offered a far less variegated media landscape than existed earlier in these countries. But communications flourished in these areas, if highly unevenly, through numerous means, in the Soviet Bloc even after the 1961 erection of the Berlin Wall. (Inhabitants could also receive radio and, for those with a set, television broadcasts from bordering countries.) Compare Wolfgang Eichwede, *Samizdat: Alternative Kultur in Zentral- und Osteuropa—Die 60er bis 80er Jahre* (Bremen, 2000).

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lenging tight constraints on public behavior and self-expression. This was the message to observers of the carnivalistic "blizzard" actions of the "Freedom and Peace" (WiP) movement in 1980s Poland, of the public antics of Czechoslovakia's "Society for a Merrier Present," and of the cluster of Bratislavans who simply stood together in public for thirty minutes as police officers gathered.[18] "Happenings," humor, and the *détournement* of symbols were all part of the repertoire. Postwar popular politics was not only protest (and was not always oppositional, although that is the focus here); nor was it necessarily public in a conventional sense. It could take place "behind the scenes," creating new spaces for communication, affording alternative modes of living, working, and interacting, and sometimes offering protection from repression. Through the creation of "alternate public spheres," its impact could still be broad. Thus the establishment of "women's centers" in countries across Europe from the 1970s provided space to test new forms of media and communications, as well as of sociability and lifestyle, often challenging the dominant power relations at many levels.[19] Many scholars have ignored such activities as politically epiphenomenal and inconsequential. By placing them in a broader context and taking a longer view, recent historical work has highlighted their significance. Whether more or less visible, this wide range of acts was overwhelmingly nonviolent, both physically and in the sense of attempting to force one's views on others.[20] Collectively these actions confronted the prevailing limits of legitimate political participation, and served more broadly to redefine "politics" and democratic participation.

Attendant to this range of political forms, these historians have also exposed the variable loci of popular politics, as well as connections across events. Scholars have long observed that the protest that originated on East Berlin's Stalin Avenue in June 1953 radiated briefly out of the city before fizzling with the arrival of Soviet tanks. Gary Bruce claims evidence for a wide variety of related popular political acts throughout the country, not just at worksites, and even in the least populated locales and those most removed from the capital. Padraic Kenney avers that to focus exclusively on the Gdansk shipyards in 1980 as antecedent to the events of 1989 in Poland is to ignore the cumulatively significant popular political acts, often initially motivated by Solidarity, that took place across the country—and well beyond its

[18] Kenney, *Carnival*, 67, 266–267, 215–217.

[19] Compare Eley, *Forging Democracy*, 371–372; Belinda Davis, "The City as Theater of Protest: West Berlin and West Germany, 1962–1983," in Gyan Prakash and Kevin M. Kruse, eds., *The Spaces of the Modern City: Imaginaries, Politics, and Everyday Life* (Princeton, N.J., 2008), 247–274; see also Kenney, *Carnival*, 33, 104; Sven Reichardt, " 'Wärme' als Modus sozialen Verhaltens—Vorüberlegungen zu einer Kulturgeschichte des linksalternativen Milieus vom Ende der sechziger bis Anfang der achtziger Jahre," *vorgänge* 44, no. 171/172 (2005): 175–187; Adelheid von Saldern, "Markt für Marx: Literaturbetrieb und Lesebewegungen in der Bundesrepublik in den Sechziger- und Siebzigerjahren," *Archiv für Sozialgeschichte* 44 (2004): 149–180. Still more historical work on such themes is welcome. Neither "subcultures" nor organizations and informal groupings, however explicitly political, were new to the postwar era, any more than were demonstrations.

[20] There was, to be sure, violent popular political activity in postwar Europe. The literature discussed here focuses predominantly on actions that were not physically injurious, commensurate with their proportional contemporary dominance. This is an important corrective to the imbalanced recent scholarly attention to popular political violence. At the same time, it would be useful if violence were addressed more directly in some of these works. Activists were also the object of violence and repression, in Western as in Eastern Europe. Compare Belinda Davis, "Jenseits von Terror und Rückzug: Politischen Raum und Verhandlungsstrategien in der BRD der 70er Jahre," in Heinz-Gerhard Haupt et al., eds., *Innere Sicherheit und Terrorismus in der Bundesrepublik der 1970er Jahre* (Frankfurt, 2006), 154–186.

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FIGURE 1: Call to a protest drumming in East Berlin, East Germany, in June 1989. This unusual form of protest was called by the "Church from Below," an oppositional grouping active since 1987. The drumming, which took place over six consecutive days, was held to protest the recent repression of protesters in Tiananmen Square, China, but was clearly also addressed to East German government leaders, at a moment of significant popular opposition to the perceived mock elections of May 1989. Image reproduced courtesy of the Robert-Havemann-Gesellschaft.

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FIGURE 2: Mural in Freetown Christiania, Copenhagen, Denmark. Christiania is an alternative living community and semi-autonomous self-governing unit that grew up in 1971 as homeless people and other area residents claimed the vacant land formerly occupied by a military barracks. As a symbol of relatively successful social and economic experimentation in Denmark and more broadly, it has played a role for those outside the community as well as within. Image reproduced courtesy of Wikimedia Commons, Copenhagen_entrance_christiania.jpg.

borders. Some East German activists seized upon the "intensity of nationalism" in Poland as a means to mobilize others in their own country. Greek activists sought to create a "sea of peace" in the mid-1970s, winning over some of their resistant Mediterranean neighbors. Some contributors to *Il lungo decennio* find that Italians' communications with French people, both prominent and not, and their longstanding cultural ties contributed to the Italian women's and gay rights movements, antecedents of which were emerging by the early 1960s.[21] Although Europe was the birthplace of and has provided enduring fertile ground for nationalism, it has also

[21] Kenney, *Carnival*, 111; Wittner, *Nuclear Abolition*, 24; Roberta Fossati, "Un 'sogno di fusion perfetta': Il mondo cattolico e la politica dei sessi," in Adagio et al., *Il lungo decennio*, 73–90; Dario Petrosino, " 'I padri italiani non dormono più': Il nascere della 'questione omosessuale' nell'Italia degli anni sessanta," ibid., 91–108. Compare variously Mariana Hausleitner, "Die Auswirkungen der ungarischen Revolution von 1956 in Rumänien," Conference Die Ungarische Revolution 1956 Kontext—Wirkung—Mythos, Berlin-Brandenburgische Akademie der Wissenschaften, Berlin, October 4–6, 2006; and Hausleitner, "Die Umbrüche in Mittel- und Südosteuropa," in Egon Bahr et al., eds., *Friedensgutachten 1990* (Hamburg, 1990), 97–109; Holger Nehring, *The Politics of Security: West European Protests against Nuclear Weapons and the Cold War* (Oxford, forthcoming); Kristina Schulz, *Der lange Atem der Provokation: Die Frauenbewegung in der Bundesrepublik und in Frankreich, 1968–1976* (Frankfurt, 2002); Gerd-Rainer Horn, "The Working-Class Dimension of 1968," in Horn and Padraic Kenney, *Transnational Moments of Change: Europe, 1945, 1968, 1989* (Lanham, Md., 2004), 95–118; Muriel Blaive, *Une déstalinisation manquée: Tchécoslovaquie 1956* (Brussels, 2004).

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exposed the limits of national boundedness, as a continent of many relatively small countries, historic regional traditions, frequent border shifts, and, especially after 1945, active migration. From the 1950s, a range of factors also contributed to the emergence of a "European" identity. These and other phenomena encouraged the forging of transnational contacts that played a prominent role in postwar European popular politics.[22]

Such ties had a political impact even through the "Iron Curtain," despite formidable repression and danger. There was, for example, a substantial—and multidirectional—exchange of information, ideas, and inspiration between Czechoslovaks and Western Europeans during the "Prague Spring" of 1968. Direct, personal relationships engendered hardships, cultural and psychological as well as governmental. Czechoslovak and West German student activists, respectively, often felt frustrated by a sense that the other group did not understand their situation. Nonetheless, these Czechoslovaks saw their activism as performing a "European" identity, binding them to their Cold War opposite numbers.[23] They claimed that as Europeans, they had a right to liberty, democracy, and meaningful political participation. Definition of these terms was highly variable, as these activists discovered. This claim was informed, moreover, by a selective reading of European history. But it was of consequence that these Czechoslovaks chose to draw on this view of Europe, which was borrowed from outside their country as well as from their own past, and to work in turn to advance it. While Soviet tanks on the streets of Prague in August 1968 cast a pall across the continent over hopes for this "Europe," cross-continental political contacts actually flourished in the 1970s and 1980s. Activists across this divide continued to encounter frictions, despite the larger goals they claimed to share. In 1982, Hungarian Dialogue activist Ferenc Köszegi invited historian and Campaign for Nuclear Disarmament (CND) co-founder E. P. Thompson to speak to peace protesters in a crowded Budapest living room, with the tacit understanding that Thompson was not to mention his identification as a socialist.[24] And although

[22] On migration within Europe, see, e.g., Alfred Rieber, ed., *Forced Migration in Central and Eastern Europe, 1939–1950* (New York, 2000); Pertti Ahonen, *After the Expulsion: West Germany and Eastern Europe, 1945–1990* (Oxford, 2003); Karen Schönwälder et al., eds., *European Encounters: Migrants, Migration, and European Societies since 1945* (Aldershot, 2003); Anke Asfur and Dietmar Osses, eds., *Neapel—Bochum—Rimini: Arbeiten in Deutschland—Urlaub in Italien* (Essen, 2003). On European identities, see variously Francesco Germinario, "Con Evola, oltre Evola: Europeismo, riattualizzazione del nazismo e nuova identità politico-culturale della destra," in Adagio et al., *Il lungo decennio*, 345–372; Ute Frevert, *Eurovisionen: Ansichten guter Europäer im 19. und 20. Jahrhundert* (Frankfurt, 2003); Tony Judt, *A Grand Illusion? An Essay on Europe* (New York, 1996). This emerged in part as an alternative to American- and Soviet-affiliated identities, if sometimes in paradoxical form.

[23] Paulina Bren, "1968 East and West: Visions of Political Change and Student Protest from across the Iron Curtain," in Horn and Kenney, *Transnational Moments*, 119–137. On continent-wide response, see Karel Bartošek, "Rencontre inattendue en Tchécoslovaquie," in Dreyfus-Armand, *Les Années 68*, 299–312; Wolfgang Engler, "Konträr und parallel: 1968 im Osten—Thesen," in François et al., *1968*, 105–109; Jerzy Eisler, "March 1968 in Poland," in Fink, *1968*, 237–251; Maud Bracke, "The 1968 Czechoslovak Crisis: Reconsidering Its History and Politics," *Contemporary European History* 12, no. 3 (August 2003): 373–383; and the interdisciplinary collection François Fetjö and Jacques Rupnik, eds., *Le Printemps Tchécoslovaque: 1968* (Brussels, 1999), which also emphasizes European identity.

[24] Patrick Burke, "A Transcontinental Movement of Citizens? Strategic Debates in the 1980s Western Peace Movement," in Horn and Kenney, *Transnational Moments*, 189–206; see also Wittner, *Nuclear Abolition*, 217; Kenney, *Carnival*, 119, and on broad continental peace work and related tensions and suspicions, ibid., chap. 3: "As If in Europe," 91–120. See too Miroslav Vaněk, "The Development of a Green Opposition in Czechoslovakia: The Role of International Contacts," in Horn and Kenney, *Transnational Moments*, 173–188; and, especially on East Germany, references in Uta G. Poiger, *Jazz, Rock,*

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an acknowledged key to the growth of the peace movement in the 1980s was its ability to reach across the usual political lines, Dutch peace protesters swallowed hard when it came to supporting allied Polish reform activists, who held up placards honoring Ronald Reagan. But such bonds continued to burgeon, often in the name of European interests and values.

If a post-1989 perspective has shed new light on both the power and the limits of the Cold War, it has also made contemporary historians more attentive to connections across a "globalized" world. In the context of popular politics, there is evidence of communications between a wide range of activists within and beyond Europe.[25] From the late 1950s, for example, many British, French, and West German activists took their cues, in terms of both content and forms of protest, from Africans who had come to Western Europe to work or study. Scholars have long written about significant American influences of all sorts in postwar Western Europe. But close connections with individual Americans were also a source of the heavy West European protest against U.S. involvement in Vietnam, as local students and others made contact with visiting American students and with GIs, or traveled to the United States themselves. (The informal politics of ethnic minorities and migrant communities, intra-European and otherwise, have attracted only limited historical attention.)[26] By the early 1980s, these global—personal—connections, along with formal political policy shifts, were central to the simultaneous rise of massive peace and anti-nuclear protests across Europe and around the world. Economic globalization, post-industrialism, international neo-liberalism, and neo-capitalism all created conditions that contributed to political mobilization in Eastern Europe in the years before 1989.[27] But contemporary European activists interpreted and responded to pre-

and Rebels: Cold War Politics and American Culture in a Divided Germany (Berkeley, Calif., 2000); Jacco Pekelder, *Nederland en de DDR: Beeldvorming en betrekkingen, 1949–1989* (Amsterdam, 1998); Arnd Bauerkämper et al., eds., *Doppelte Zeitgeschichte: Deutsch-deutsche Beziehungen 1945–1990* (Bonn, 1998); Bauerkämper, ed., *Britain and the GDR: Relations and Perceptions in a Divided World* (Berlin, 2002).

[25] Wittner, *Nuclear Abolition*; Klimke, *The "Other Alliance"*; Maria Höhn, "Black Panther Solidarity Committee and the Trial of the Ramstein 2," in Davis, *Changing the World*; Belinda Davis, "A Whole World Opening Up: Transcultural Contact, Difference, and the Politicization of New Left Activists," ibid.; on connections often less personal, see Wilfried Mausbach, *Vietnam in Deutschland: Die Bedeutung des amerikanischen Indochinakrieges für Politik und Gesellschaft in der Bundesrepublik* (forthcoming); Robert Frank, "Imaginaire politique et figures symboliques internationales: Castro, Hô, Mao, and le 'Che,'" in Dreyfus-Armand, *Les Années 68*, 31–48; Quinn Slobodian, "Radical Empathy: The Third World and the New Left in 1960s West Germany" (Ph.D. diss., New York University, forthcoming). For examples of work by non-historians, see Jennifer Hosek, "Cuba and the Germans: A Cultural History of an Infatuation" (Ph.D. diss., Stanford University, 2004); Ingo Juchler, *Die Studentenbewegungen in den Vereinigten Staaten und der Bundesrepublik Deutschland der sechziger Jahre: Eine Untersuchung hinsichtlich ihrer Beeinflussung durch Befreiungsbewegungen und -theorien aus der Dritten Welt* (Berlin, 1996).

[26] But compare Jesús Casquete, *Política, cultura y movimientos sociales* (Bilbao, 1998); Amaia Lamikiz Jauregiondo, "La perspectiva local en el estudio de la sociabilidad: Espacios asociativos de la juventud guipuzcoana en la década de 1960," *Vasconia: Cuadernos de historia—geografía* 33 (2003): 49–61; Jan Motte and Rainer Ohliger, eds., *Geschichte und Gedächtnis in der Einwanderungsgesellschaft* (Essen, 2004); Eva Schandevyl, "Het Brusselse vakbondswezen en de migranten/allochtonen: Discours en syndicale structuren sinds de jaren 1970–80," in E. Witte and J. Degadt, eds., *Politiek, taal, onderwijs en samenleving in beweging* (Brussels, 2005), 383–441; Jennifer Miller, "Postwar Negotiations: The First Generation of Turkish 'Guest Workers' in Germany, 1961–1973" (Ph.D. diss., Rutgers University, forthcoming).

[27] Jarle Simensen, "The Global Context of 1989," in Horn and Kenney, *Transnational Moments*, 157–172; compare Urso, "Il lungo decennio"; and for sociological approaches, Immanuel Wallerstein,

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vailing trends and events through direct or indirect connections with others, outside as well as within the region and continent. British public scholar Timothy Garton Ash remembers that for him, "Pierre in Paris, Helena in Warsaw, John in Washington, and Michael in Bonn were, and remain, friends" and "were a 'we,' " constituting an ongoing "political community."[28] Such mutual influences have many well-studied precedents, not least in the uprisings of the eighteenth and nineteenth centuries in Europe and elsewhere, even before the breadth and rapidity of postwar media and communications. Critical here is how this pushes us to read postwar "European" political history somewhat differently.

Garton Ash's comment hints at another thread that runs through these examples, one that differentiates the work of these historians from that of many social scientists. Political action and interaction across space and across time were directly intertwined. Lawrence Wittner claims that it was the steady drumbeat of peace and anti-nuclear protest in different countries across decades that informed officials' restraint about detonating nuclear weapons and their willingness to adopt arms limitations. Just as important, over time these acts were significant for activists and would-be activists themselves. Whether large or small, local or far-flung, more or less recent, conventional, or "successful," and frequently irrespective of particular goals, such actions together often served as a fillip to popular political activity, through individual and collective memory.[29] Italy's "Red Biennial" of 1919–1920 spurred activism in that country—and also in France—in the 1960s and 1970s. The "Prague Spring," including its widely lamented repression, forged a "common European memory" on which many across the continent drew in subsequent decades.[30] Padraic

"1968, Revolution in the World-System: Theses and Queries," *Theory and Society* 18 (1989): 431–449; Giovanni Arrighi, Terence K. Hopkins, and Immanuel Wallerstein, "1989: The Continuation of 1968," *Review* (Fernand Braudel Center for the Study of Economies, Historical Systems, and Civilizations) 15, no. 2 (Spring 1992): 221–242.

[28] Timothy Garton Ash, *Free World: Why a Crisis of the West Reveals the Opportunity of Our Time* (New York, 2004), 7; also Garton Ash, *We the People: The Revolution of '89 Witnessed in Warsaw, Budapest, Berlin and Prague* (London, 1990). Compare social science "frame analysis."

[29] Eley, *Forging Democracy*, focuses significantly on memory. See too Søren Hein Rasmussen, *Saere alliancer politiske bevaegelser i efterkrigstidens Danmark* (Odense, 1997); Árpád von Klimo, "St. Stephen's Day: Politics and Religion in 20th-Century Hungary," *East Central Europe = L'Europe du centre-est* 26, no. 2 (1999): 15–31; Wolfgang Kraushaar, *1968 als Mythos, Chiffre und Zäsur* (Hamburg, 2000); Temma Kaplan, "Demonstrating to Remember in Spain," in Kaplan, *Taking Back the Streets: Women, Youth, and Direct Democracy* (Berkeley, Calif., 2003), 176–202; Andrzej Paczkowski, *The Spring Will Be Ours: Poland and the Poles from Occupation to Freedom* (University Park, Pa., 2003); István Rév, *Retroactive Justice: Prehistory of Post-Communism* (Stanford, Calif., 2005); Kowalczuk, *Der Tag X*; Danielle Tartakowsky, *Les manifestations de rue en France, 1918–1968* (Paris, 1998); Padraic Kenney and Max Paul Friedman, *Partisan Histories: The Past in Contemporary Global Politics* (New York, 2005); Stefan Troebst, *Postkomunistyczne kultury pamięci w Europie Wschodniej: Stan, kategoryzacja, periodyzacja* (Wrocław, 2005); Troebst, ed., *Diktaturbewältigung, Erinnerungspolitik und Geschichtskultur in Polen und Spanien* (Munich, 2003); Belinda Davis, " 'Everyday' Protest, Violence, and the Culture of Conflict in Berlin, 1840–1980," in Christof Mauch and Andreas Daum, eds., *Berlin—Washington, 1800–2000: Capital Cities, Cultural Representation, and National Identities* (Cambridge, 2005), 263–284; Bernd-Rainer Barth, "Ungarn 1956: Denkmälersturz und Metamorphosen," *Horch und Guck* 56 (2006): 35–36. Compare on the U.S. Jacqueline Dowd Hall, "The Long Civil Rights Movement and the Political Uses of the Past," *Journal of American History* 91, no. 4 (March 2005): 1233–1263. There is a substantial theoretical literature on different forms of memory.

[30] Jacques Rupnik and François Fetjö, "Conclusion: 1956–1968–1980–1989," in Fetjö and Rupnik, *Printemps*, 343–345, here 343. Social scientists increasingly address memory in popular politics. Compare M. Siisiainen, "Social Movements, Voluntary Associations and Cycles of Protest in Finland 1905–91,"

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Kenney asserts that although Ukrainian protesters in the 1980s were avowedly future-oriented, they drew inspiration from nationalist activists in World War II. Other kinds of events also spurred (or inhibited) activism. The 1979 Russian invasion of Afghanistan, the 1980 assassination of John Lennon, the 1986 nuclear accident in Chernobyl, all played some role in catalyzing protest in Poland in the late 1980s. Certainly the memory of fascism, Nazism, genocide, and World War II played a significant role in postwar activism, although that role is given variable attention in these new studies.[31] Popular political participation thus developed to some extent through individual and collective memory, ultimately that of millions of people. In turn, this regular "replaying" of the right to political self-expression through broad means became an important avenue to "creating democracy," and to motivating further acts beyond as well as within formal political channels. Historians have long attended to the role of memory across the centuries, including its function as a catalyst in popular politics. Significant here is the way that attention to memory, across large and variable populations, helps to complicate both more abstract and top-down characterizations of "democratization" in postwar Europe and those focusing on singular moments such as "'68" and "'89."

An emphasis on breadth of population also makes this narrative new and significant. Postwar European popular politics has sometimes been characterized as the actions of "tiny, radical minorities," particularly of "elites," such as students (and otherwise by more conventional populations involved in protest, such as workers, most often in the form of "economic" protest). By taking a capacious view of informal politics, and by looking not just beyond national boundaries but also across more conventional cultural barriers, these historians have found activists of less predictable types. Youth protest was vital well beyond university students; protest by much older actors was significant as well.[32] For Bruce and Kenney, to ignore rural pop-

Scandinavian Political Studies 1 (1992): 21–40; Jan Kubik, *The Power of Symbols against the Symbols of Power: The Rise of Solidarity and the Fall of State Socialism in Poland* (University Park, Pa., 1994); Paloma Aguilar Fernández, *Memoria y olvido de la guerra civil española* (Madrid, 1996); Christos Zapheires, *Emeis tou '60 hoi ekdromeis* (Athens, 2000); on the "afterlives" of contemporary watersheds, see Kieran Williams, *The Prague Spring and Its Aftermath: Czechoslovak Politics, 1968–1970* (Cambridge, 1992); Paulus Ebner and Karl Vocelka, *Die zahme Revolution: '68 und was davon blieb* (Vienna, 1998); Morten Bendix Andersen and Niklas Olsen, eds., *1968—dengang og nu* (Copenhagen, 2004); Remigijus Riekašius, "Protesto Politika Lietuvoje," *Politologija* 2 (2003): 127–146; Andrea Genest, "Die Krise von 1968 in der Bewertung der polnischen Opposition—Ereignisse und Rezeption vor und nach dem Systemwechsel von 1989" (Ph.D. diss., Free University Berlin, 2005). For the work of a literary critic, cf. Kristin Ross, *May '68 and Its Afterlives* (Chicago, 2002).

[31] Among key texts here, Eley addresses this head-on; see also Germinario, "Con Evola, oltre Evola"; Andrea Rapini, "Antifascismo e Resistenza nelle riviste della nuova sinistra (1960–1967)," in Adagio et al., *Il lungo decennio*, 373–411. Compare Christiane Kohser-Spohn, *Mouvement étudiant et critique du fascisme en Allemagne dans les années soixante* (Paris, 1999); Wilfried Mausbach, "'Man muß die ganze Wut diesen Herrenrassenbanditen ins Gesicht schreien': Die 68er und die nationalsozialistische Vergangenheit," *Deutschland-Archiv* 38 (2005): 273–280; Belinda Davis, "Violence and Memory of the Nazi Past in 1960s–70s West German Protest," in Philipp Gassert and Alan Steinweis, eds., *Coming to Terms with the Past in West Germany: The 1960s* (New York, 2006), 210–237; also Leonardo Paggi, *La memoria del nazismo nell'Europa di oggi* (Florence, 1997).

[32] Compare Attilio Mangano, "Capelloni e cinesi: I giovani negli anni sessanta," in Adagio et al., *Il lungo decennio*, 37–50; Marica Tolomelli and Jan Kurz, "Gli studenti: Tra azione e mobilitazione," ibid., 51–72; Andrea Genest, "Zwischen Anteilnahme und Ablehnung—Die Rollen der Arbeiter in den Ereignissen 1968 in Polen," in Bernd Gehrke and Gerd-Rainer Horn, eds., *1968 und die Arbeiter: Studien zum proletarischen Mai in Europe* (Hamburg, 2007), 185–209; Linde Apel, *Linke und rechte Politisierung:*

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ulations, or those living outside metropolises and well-studied movement "centers," would be to miss seeing major points in the longer trajectories of protest they each trace.[33] Recent work has also emphasized a startling prominence across the era and the continent of activists who were motivated in part by Christian religious identities.[34] Organized Catholicism provided an unexpectedly encouraging early source for the women's movement in Italy, for example, through Catholic women's organizations and Catholic universities; this, Roberta Fossati argues, was in contrast to the puritanical influences of American culture on Italian sexuality.[35] And well beyond the longstanding acknowledgment of the importance of the women's movement for postwar European political culture, these historians find that women (whether also as workers, students, mothers, filmmakers, or otherwise) played a pivotal role in grassroots activity throughout the postwar period, despite their frequent contemporary "invisibility," and notwithstanding their far smaller collective role in formal politics. While Eley argues women's centrality most explicitly, the consistently prominent position of women in these texts represents a major corrective to the earlier scholarly habit of unreflectively adopting dominant contemporary practices.

The actors in these myriad actions, groupings, and networks were highly varied. By the 1970s, the latter were often internally pluralistic. This is reminiscent in some respects of the interlocking "societies," of varying political effectiveness, that marked nineteenth-century Europe—societies, moreover, that often promoted transnational contact. But this character has remained out of historians' focus with respect to contemporary history, in part because of the often less official or less institutional nature of these groupings. In describing these frequently kaleidoscopic, spontaneous, and short-lived groups, the best of this scholarship suggests that real people did not usually live or act through singular identities, and, conversely, that categories of identity harbored great heterogeneity.[36] "Blackness" grew and transformed in

Die Hamburger Schülerbewegung der 1960er und 70er Jahre (forthcoming); also Poiger, *Jazz*. On the variability of protesters in 1956 Hungary, see Tibor Dömötörfi, "Im Banne der Revolution: Über die soziale Zusammensetzung der Aufständischen von 1956" (lecture, Historical Seminar, University of Zurich, November 14, 2006). "Student" activists could be a deceptive characterization, including in terms of presumptions about age; see Belinda Davis, *The Internal Life of Politics: The "New Left" in West Germany, 1962–1983* (forthcoming).

[33] Compare Arnd Bauerkämper, *Die SED und die Bauern: Agrarwirtschaft und ländliche Gesellschaft in der SBZ und DDR* (Hagen, 1999); within sociological work, Maria Kousis, "Environmental Protest Cases: The City, the Countryside, and the Grassroots in Southern Europe," *Mobilization* 4, no. 2 (Fall 1999): 223–238; Bert Klandermans et al., "Identity Processes in Collective Action Participation: Farmers' Identity and Farmers' Protest in the Netherlands and Spain," *Political Psychology* 23, no. 2 (2002): 235–251.

[34] Rocco Cerrato, "Il mondo cattolico italiano negli anni sessanta," in Adagio et al., *Il lungo decennio*, 309–328; Fossati, "Un 'sogno' "; also Benjamin Ziemann, "Zwischen sozialer Bewegung und Dienstleistung am Individuum: Katholiken und katholische Kirche im therapeutischen Jahrzehnt," *Archiv für Sozialgeschichte* 44 (2004): 357–394; Denis Pelletier, *La crise catholique: Religion, société, politique en France (1965–1978)* (Paris, 2002); Gerd-Rainer Horn and Emmanuel Gérard, eds., *Left Catholicism: Catholics and Society in Western Europe at the Point of Liberation, 1943–1955* (Leuven, 2001). Scholars have long recognized religious identities among postwar peace protesters and, in Eastern Europe from the 1970s, "reform" activists. Compare Wittner, *Nuclear Abolition*.

[35] Fossati, "Il mondo cattolico," 76. Compare for critical views Victoria De Grazia, *Irresistible Empire: America's Advance through Twentieth-Century Europe* (Cambridge, Mass., 2005); Richard F. Kuisel, *Seducing the French: The Dilemma of Americanization* (Berkeley, Calif., 1993); Konrad Jarausch and Hannes Siegrist, eds., *Amerikanisierung und Sowjetisierung in Deutschland 1945–1970* (Frankfurt, 1997).

[36] Compare for earlier periods Robert Q. Gray, *The Labour Aristocracy in Victorian Edinburgh* (Ox-

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FIGURE 3: A demonstration against sexism, racism, and nationalism in Paris, France, in 1995. Demonstrators often made their demands from a range of identities. Popular political activity among immigrant populations such as these women from Western Sahara was a regular component of informal politics in Europe from the earliest postwar years. Anti-immigrant protest also took place throughout this period. Image reproduced courtesy of Francine Bajande, Project "Traces, mémoires, histoire des mouvements de femmes de l'immigration."

1970s–1980s Britain as an identity of political opposition.[37] Many of the "political intellectuals" and "new intellectuals" in France in the 1960s were among those who supported as well as assailed the Parisian protest of May 1968. As important, these intellectuals together contributed to a new and more critical collective memory of World War II—and of the Algerian war—that helped to inform the protest and was indeed part of it.[38] Collectively, these historians provoke a challenge to a term such as "activist," which seems to designate a limited and fixed population. But if it makes sense to look beyond a "tiny, radical minority," how many people are being de-

ford, 1976); Denise Riley, *"Am I That Name?" Feminism and the Category of "Women" in History* (Minneapolis, 1988). "Workerism" enjoyed renewed strength in Italy in the 1960s and 1970s before again losing appeal: Luigi Urettini, "L'operaismo veneto da 'Il progresso veneto' a 'Potere operaio,' " in Adagio et al., *Il lungo decennio*, 173–204.

[37] Eley, *Forging Democracy*, 473.

[38] Brillant, *Les Clercs de 68*; Ingrid Gilcher-Holtey, *"Die Phantasie an die Macht": Mai 68 in Frankreich* (Frankfurt, 1995); also Jean-François Sirinelli, ed., *Les générations intellectuelles: Effets d'âge et phénomènes dans le milieu intellectuel français* (Paris, 1987). Compare William Gambetta, "Alla conquista dei 'nuovi intellettuali,' " in Adagio et al., *Il lungo decennio*, 287–308; Siegward Lönnendonker, ed., *Linksintellektueller Aufbruch zwischen "Kulturrevolution" und "kultureller Zerstörung": Der Sozialistische Deutsche Studentenbund (SDS) in der Nachkriegsgeschichte* (Opladen, 1998); John Abromeit, "Marcuse, Adorno, and the New Left," in Davis, *Changing the World*; Jacco Pekelder, " 'Inleiding' en 'Het geweld der kameraden: Peter Brückner, het terrorisme en de linkse identiteit,' " in Pekelder and Frits Boterman, eds., *Politiek geweld in Duitsland: Denkbeelden en debatten* (Amsterdam, 2005). For a sociologist's view, see John Torpey, *Intellectuals, Socialism, and Dissent: The East German Opposition and Its Legacy* (Minneapolis, 1995).

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scribed? Millions of Europeans mobilized against nuclear buildup in the early 1980s. More generally, we have a less clear sense of numbers. We might venture still that a sense of the possibility and legitimacy of such activism, by "normal people" on a regular basis—a sense little sanctioned either by formal politics or by the dominant political cultures across the continent in the first postwar decades—was gained in part from the relatively regular and often effective (if less dramatic and less populous) protest of the preceding decades.[39]

INDIVIDUALLY AND COLLECTIVELY, THESE ARE COMPELLING FINDINGS about the patterns of popular political practice in postwar Europe. They raise a raft of questions, not least pertaining to the functionality of modernist political terminology, and of the models of politics these terms connote, regarding the nature of political change and the categorization of political actors. Social scientists are among those who have begun to work through such questions. Historians stand to contribute much to this discussion, for example, by attending to the vector of time, and by challenging static categories and abstract models. The historians discussed here seem to have adopted their vocabularies more and less carefully, but even the most attentive and judicious fall back on terms that threaten to challenge the implications of their own evidence and assertions. Yet it is not necessarily sufficient to simply avoid particular characterizations. To see these problems, let us choose some sample terms—"revolution" and political "left" and "right"—and briefly sketch out their implications in these works. To start with, the notion of "revolution" is regularly invoked in much of this historical work, but its meaning is not always transparent—or transparently useful. The modern notion of political revolution emerged out of progressivist political models, which many of these historians eschew. A more secular use refers more simply to accelerated, radical change (however lasting). But is this enough to tell us anything? How well does the term match up with these scholars' characterization of grassroots politics in Europe after 1945? What might their work on Europe, where the term originated, suggest for revolution as a heuristic?

Some of these scholars use "revolution" enthusiastically, but threaten to explode its meaning, descriptively and analytically. A core tenet and the central provocation of Gary Bruce's study is that the East German uprising centered on June 17, 1953, was a revolution. As evidence, he cites the fact that protest moved out from workers in East Berlin, and their limited demands, to encompass broader populations, who were calling for free elections. He argues, "Fruitlessness . . . cannot be a criterion of determining whether or not something happened."[40] But as Bruce demonstrates, East Germans had been advancing this call from the time the Communist Party

[39] Greater attention to numbers could be useful. A sense of the normalcy of regular, widespread informal political engagement informs some political theorists' definition of robust democracy, as in Iris Marion Young, *Inclusion and Democracy* (Oxford, 2000).

[40] Bruce, *Resistance*, 8, here with reference to Nazi-era resistance, but with clear meaning for his case as well. Hannah Arendt, *On Revolution* (1962), raises the issue of aftermaths: on this basis she controversially found the French revolution to have "failed" in contrast with the American. Still, she challenged what she perceived as the U.S. "voter democracy" as a limited fulfillment of revolutionary aims. On defining revolution, see Jack Goldstone, "Towards a Fourth Generation of Revolutionary Theory," *Annual Review of Political Science* 4 (2001): 139–187.

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FIGURE 4: Protesters demand bread in Poznan, Poland, in June 1956. Alongside the better-known Hungarian uprising, or "revolution," that year, there were regular challenges elsewhere in Eastern Europe as well, including this Polish city. Although this call for bread was concrete and specific, it also represented broad criticism of the government. While this Hungarian protest was ultimately definitively crushed, Polish protesters (usually characterized as workers, but representing a wider population) brought about a "de-Stalinization" of the Polish regime. This photo was taken by a police agent. Image reproduced courtesy of Agencja Fotograficzna Ośrodka KARTA, Warsaw.

(SED) first began tightening its stranglehold on the political process. His achievement is in making the case for a steady pattern of noisy refusal across the decade.[41] So what was the "revolution" of 1953? What was the change, and what the model of change? There is little sense of what this protest did or did not achieve over time, including in transforming the protest culture itself, and in winning positive response from various authorities. This responsiveness, which we know of from other recent work, suggests dynamic if not linear patterns of transformation, which are lost in Bruce's static model of "resistance and repression," making it sound as if nothing

[41] See also Wittner, *Nuclear Abolition*, 36; Poppe, *Zwischen Selbstbehauptung*; Anke Silimon, *"Schwerter zu Pflugscharen" und die DDR: Die Friedensarbeit der evangelischen Kirchen in der DDR im Rahmen der Friedensdekaden 1980–1982* (Göttingen, 1999); Andrew Port, "When Workers Rumbled: The Wismut Upheaval of August 1951 in East Germany," *Social History* 22, no. 2 (1997): 145–174.

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changed at all.[42] If official response was indeed inconsequential until the sudden fall of the government in 1989, and if the protest itself preceded and postdated the events of June 17 by years, what makes 1953 a revolution? What was the relationship between protest in the 1950s and protest in the 1980s? Between even the latter and the fall of the regime? The use of "revolution" here obscures the delicate processes of influence and causality that Bruce seems to imply.

Others exhibit more discomfort with the term as applied broadly to contemporary Europe, even as they try to salvage it. Geoff Eley avers at the outset of his comprehensive study that for the postwar and the longer period running from the nineteenth century through the twentieth, "the most important gains for democracy have only *ever* been attained through revolution"; but he continues, "or at least via those several concentrated periods of change I'll call the great constitution-making conjunctures of modern European history."[43] (Naturally the meaning and function of "democracy" can be and have been challenged.)[44] Certainly these conjunctures, constitution-making and otherwise, including 1776–1815, 1859–1871, 1914–1923, 1943–1949, and 1989–1992, were periods of great change, often in many directions at once, and engendering in each case new patterns of governance and new political expectations in some European nations, with implications for the continent. But these periods are an uneven fit with "revolution" as conventionally employed, all the more precisely in the context of the decades-long transformation of politics on which Eley focuses following the conjuncture 1943–1949. To be sure, historians of revolution refer to lengthier processes of change before and after these "big moments." Changes in democratic practice across the postwar decades are evident in Eley's study. That great changes took place in the context of conditions created by the 1945 military solution and the concomitant necessity for new government structures is manifest. That domestic antifascist activists played an important role and also formed (if contentiously) one important model for their successors in popular politics is also convincing. That change takes place at variable levels of speed and intensity is irrefragable. But how, exactly, does seeing 1943–1949 specifically as a "revolution" help us to understand processes of democratic expansion in the postwar

[42] Bruce, *Resistance*, e.g., 167. The flush of recent work on a range of topics related to Eastern Bloc countries makes clear that these regimes survived only by responding "positively" as well as "negatively" at some level to popular demands, from those concerning consumer goods to religious rights. It would be very useful to understand how such a response, or even its lack, figured into any trajectory of encouraging continued demands—and how, if at all, this related to the regime's fall. Compare variously Padraic Kenney, *Rebuilding Poland: Workers and Communists, 1945–1950* (Ithaca, N.Y., 1997); Mark Pittaway, "Control and Consent in Eastern Europe's Workers' States, 1945–1989," in Clive Emsley et al., eds., *Social Control in Europe*, vol. 2: *1800–2000* (Columbus, Ohio, 2004), 343–367; Christiane Brenner and Peter Heumos, eds., *Sozialgeschichtliche Kommunismusforschung: Tschechoslowakei, Polen, Ungarn und DDR 1945–1968* (Munich, 2005); János M. Rainer and György Péteri, eds., *Muddling Through in the Long 1960s: Ideas and Everyday Life in High Politics and the Lower Classes of Communist Hungary* (Trondheim, 2005); Katherine Pence and Paul Betts, eds., *Socialist Modern: East German Everyday Culture and Politics* (Ann Arbor, Mich., 2007).

[43] Eley, *Forging Democracy*, x. The reference to constitution-making evokes the Aristotelian definition of revolution.

[44] For exciting efforts to avoid the problems of that notion, see Margaret E. Keck and Kathryn Sikkink, *Activists beyond Borders: Advocacy Networks in International Politics* (Ithaca, N.Y., 1998); Mary E. Hawkesworth, *Globalization and Feminist Activism* (Lanham, Md., 2006). There are clearly many terms, including of my own use here, that could be challenged. On modernist political terminology, see also Keith Michael Baker and Peter Hanns Reill, eds., *What's Left of Enlightenment? A Postmodern Question* (Stanford, Calif., 2001).

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period, especially in light of his evidence for the need to continually make and remake democracy? Eley himself rues historians' "extremely constricting framework of 'revolution' versus 'reform,'" and finds compelling activists' own efforts by the 1970s to transcend such notions.[45] It is clear that he has engaged seriously with the term, but it is less clear what maintaining it does for his own analysis. More important here again is how the term threatens to eclipse the processes of change that Eley explicitly draws out, or to intimate their value primarily as the aftermath of or the precursor to the next "big moment."

Looking at 1989, the most conventionally accepted instance of revolution in postwar Europe, Padraic Kenney embraces the term. Yet he challenges the role of standard defining features. The revolution was represented not by the toppling of regimes, not just by "turning points," but rather by an "atmosphere of limitless possibility and colorful aspiration."[46] This is a seductive and thought-provoking idea, not least for the way Kenney traces this feeling among growing groups of "ordinary" people. But how well does this work as a general definition of revolution? He includes the Russian Revolution in this context, even though it is often described as having been propelled by overwhelming misery, and marked by ambivalence at best in the aftermath. Preceding 1989, Kenney sees this atmosphere as both effect and cause of an ongoing radicalization. Thus, for example, he traces the line from Polish activists' anti-alcohol campaigns and calls for privatization of social support to "genuine national consciousness and Catholic faith," which represented, Kenney claims, a wide-ranging desire for democracy and freedom. He acknowledges the limits of the causality between activists' actions and the fall of East European regimes, as others have argued. He notes, too, the widespread dissatisfaction with the results after 1989—and indeed his own subjects' challenge to the term "revolution." If many were moved to continue pushing for change after 1989, was this also the revolution? Or was the disillusionment that many experienced after 1989 the end of the revolution? Kenney's definition evokes the notion of "imagination in power," which was prominent in the 1960s.[47] One can argue that an openness to, embrace of, and belief in the possibility of change—and of one's own role in realizing such change—was characteristic at points across the postwar era, and might move us toward a better understanding of that change. But does this itself define revolution, and if so, to what end? And what are the specific implications for processes of change?

Questions about the use of such terms are not just semantic. These scholars demonstrate that popular politics in postwar Europe was often marked by protracted efforts to convince others of an idea (including of the virtue of broad political participation), or by painstaking attempts to build alternatives, and that it was accompanied by unpredictable and often relatively long-term processes of change in people's heads. This work collectively raises questions about the explicative force of

[45] Eley, *Forging Democracy*, 295.

[46] Kenney, *Carnival*, 2–3 (but compare p. 16, where he seems to advance a far more conventional definition).

[47] Ibid., 61, 7–10. Compare Eley, *Forging Democracy*, 342, 235, on "leaving normal" and "intoxicating" moments; Gilcher-Holtey, *"Die Phantasie an die Macht"*; and from a literary critic and journalist, respectively, see Sabine von Dirke, *All Power to the Imagination: The West German Counterculture from the Student Movement to the Greens* (Lincoln, Neb., 1997); and Antoine Verbij, *Tien rode jaren: Links radicalisme in Nederland, 1970–1980* (Amsterdam, 2005). The question is what makes this "revolution."

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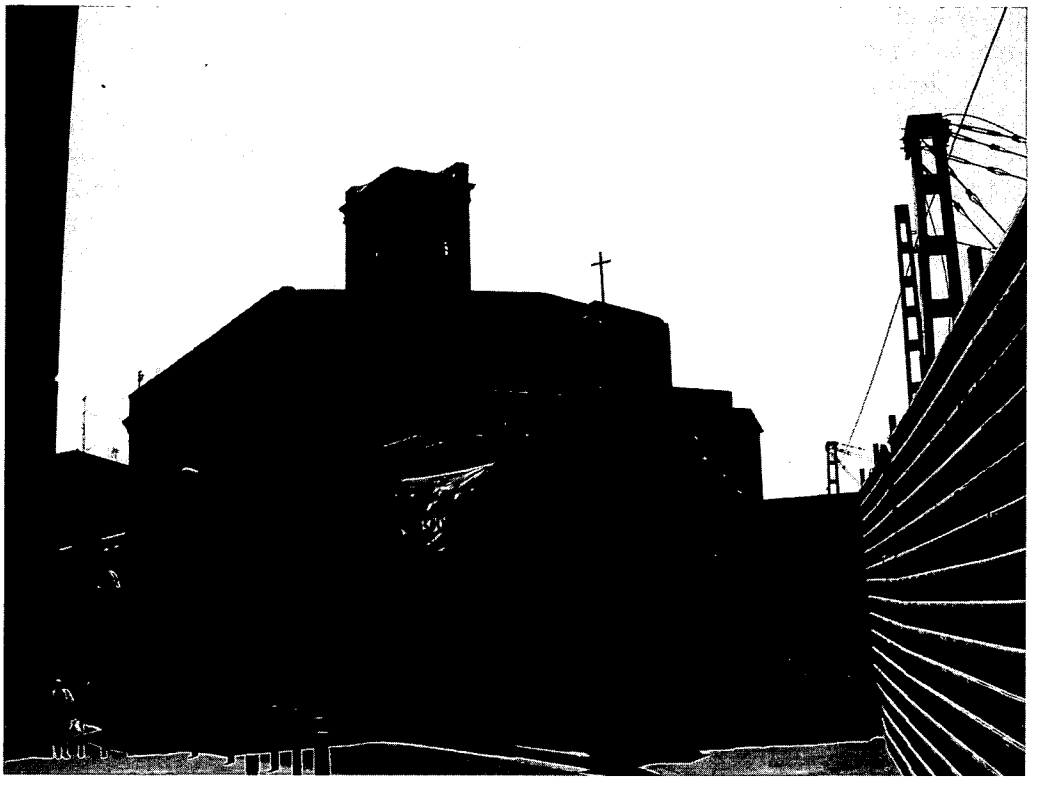


FIGURE 5: The "squatted" social and cultural center "Can Vies" in Barcelona-Sants, Spain. The text on the façade includes the incitement "Open your eyes . . . Never shut up, and cry loudly . . . Give yourself everything." By the late 1960s, squatting became one solution to the enduring postwar housing shortages in Europe. Squatted buildings were frequently sites of ongoing popular politics as well as sources of neighborhood renewal, offering space inside and out for lifestyle experimentation, alternative community cultural activities, and the communication of a range of political concerns, including those of ethnic minorities such as the Catalan. Image reproduced courtesy of Wikimedia Commons, Barcelona_okupa_Can_Vies.jpg.

"revolution" more broadly (as of "reform"). Such a notion can tell a certain story—for example, that sudden change, violent change, great change, or any change at any moment is intrinsically good or bad, predestined, or tied to particular ideologies, or that this is the means through which a broader population really makes a difference. The "moral" of that story may in this way move beyond a historian's control. We might investigate this issue with the example of one more set of terms that draws on modernist political frameworks: "left" and "right" politics. The many relevant issues include subjects' own political identities, and the relation of ideologies and practices, and of content versus form. Some of these historians may simply insist that the terms hold too much weight, which can threaten to obliterate their works' subtler and more differentiated observations. Others very deliberately circumvent such reference, with challenging results, but still leaving open questions. In this instance, too, contemporaries themselves challenged the terminology, as wedded to atavistic frameworks. These terms may well still be valuable, beyond an exploration of contemporaries' own use of them. It is very attractive to think they can be, including in the context of the relationship of the past to the present; certainly some method of characterization is necessary. But these studies raise as many questions as they answer.

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Tracing a coherent political left across the continent and over time, specifically in terms of the relative consistency of democratic advancement (and, in the postwar era, pluralism), is Geoff Eley's explicit aim.[48] This effort offers one response to historians who aver that the history of communism full stop in Europe, as well as the fascist past, has rendered a dubious verdict against Europe, for nearly the whole of the century. Eley seeks to demonstrate how this historical left forged a long-term pattern for others to draw on, in defense against renewed authoritarianism, such as authoritarian communism, and in continuing democratic expansion. Concerning the long stretch before World War II, Eley concentrates on the role of social democrats broadly conceived, especially their role out of political power. If in the postwar period these parties largely passed the torch as the leading "democratizers," Eley finds, broad groupings such as feminists, Greens, and peace activists took up that torch in Western Europe; in Eastern Europe, feminists and organizations such as Solidarity played a similar role. Yet Western European Greens frequently questioned their fit on this spectrum, explicitly challenging the adequacy of the metaphor itself, as did many feminists. Many West European peace protesters denied such a positioning for themselves. These activists' counterparts in Eastern Europe overwhelmingly rejected the label "left," finding it permanently stained by the Eastern Bloc governments in power. Certainly historians should use their perspective to make characterizations and interpretations. But if it is the role of these groups in advancing democracy that Eley seeks to emphasize, and if they themselves inconsistently embraced the characterization, what then does "left" actually tell us?

The contributors to *Il lungo decennio* do not explicitly engage with the terms themselves, in which respect they are more typical of the broader literature. But significantly, they do challenge the Italian "sixties movement" as an exclusively "left-wing" phenomenon, treating a range of informal political actors and especially of influential ideas and ideologies that all played a meaningful role, once again fitting with difficulty on the conventional spectrum. In the anthology's introduction, however, Simona Urso expresses her concern about contests over the representation specifically of the New Left, the broad grouping whose adherents, across Europe and beyond, sought to re-create a "democratic left" in contrast to Stalinism, and which some have characterized as dogmatic or "left terrorist." Like other of these historians, these contributors describe common efforts by informal activists and formal party leaders (particularly those out of power), which formed a broad and effective

[48] Eley is clearly aware of political theoretical discussions of the terms, and intends a contribution to them. Challenges came from the beginning of their use in the eighteenth century; defenders most often self-identified as "leftists." Jean-Paul Sartre's interventions in the 1960s marked a rethinking among "leftists," which accelerated in the 1980s. Compare the thoughtful and widely read responses of Norberto Bobbio, *Left and Right* (Cambridge, 1996), and Anthony Giddens, *The Third Way* (Cambridge, 1998). Both acknowledge difficulties across existing parties and governing structures, European and other, in accommodating (not to mention soliciting and enabling) widespread, regular popular political participation, beyond voting, that is not simply "absorbed" or rendered nugatory; both see such participation as prospectively offering new life to basic existing formal government structures by improving them. Bobbio finds a relative embrace of equality to be the abiding defining feature of "left," a reasonably convincing argument. As often as not, leaders of formal political parties in postwar Europe have used "left" and "right" as terms of abuse, while the variability of party platforms, simultaneous widespread acceptance of a "social state," and the realities of coalition politics have left these leaders struggling to define themselves in new ways. See Harald Martenstein, "Was heißt heute links?" *Die Zeit*, no. 19 (May 4, 2006): 65. Challenge of the spectrum is not new among historians; compare Zeev Sternhell's controversial *Neither Left nor Right: Fascist Ideology in France* (Berkeley, Calif., 1986).

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"archipelago of the lefts" (plural) in Italy in the 1960s.[49] Yet such coalitions most often splintered quickly, in this instance as Italian Communist Party (PCI) leaders grew impatient with shared decision-making, indeed condemning popular activism—and moving to ally with the Christian Democratic Party (DC). In the 1990s, leaders of the new secessionist Northern League Party worked successfully with non-party activists, possibly for a longer period than had the PCI, before they, too, broke off cooperative efforts. By the measure of broadening political participation and voice, was the Northern League then "left" of the PCI? In 1970s West Germany, the Social Democratic (SPD) "reform" government oversaw the suppression of nonviolent oppositional popular political expression arguably more repressively than had the preceding regime led by the Christian Democratic Union (CDU). Was the SPD to the "right" of the CDU?

Or did the very form of the political party in postwar Europe make it incapable of being left, specifically in Eley's sense of continuing to "advance democracy" (as opposed to maintaining the status quo)?[50] Is the term "left" ("new," "new new," and other) best applied after the war to informal political groupings and their actions? These scholars collectively seem to be advancing the argument that such groupings by their actions "performed" democracy in postwar Europe, in turn legitimating a broader definition of political participation and encouraging others to act, and this is a convincing notion. But does a focus on form solve the terminological conundrum, and, more important, does it help resolve the attendant issues of political understanding? Do the forms trump the content of informal activists' own demands as left or right? What of the widespread protest in Denmark in 1973 against the ruling Social Democrats and open immigration policy? Or the massive demonstrations led by conservatives against the deceits of Socialist Hungarian prime minister Ferenc Gyurcsány in 2006? The array of demands asserted by East European activists in the 1980s pose one broad test case, as exemplified by the Ukrainian activists who stood up to the local authorities to peacefully act in the interests of recuperating a pure national language, and of "reawakening" Christianity and nationalism among those who did not know it.[51] Can attention to form help us when activists' demands themselves seem to cut across a conventional political spectrum, as when Czech demonstrators demanded democracy, liberty, and (following the "Velvet Revolution") the expulsion of Roma, or in regard to the "conservative pacifism" and "anarchist nationalism" of Poland's "Freedom and Peace" group (WiP)? This might address the difficulty in characterizing the millions of Europeans who came together to speak

[49] Sergio Dalmasso, "L'arcipelago delle sinistre: Partiti, gruppi e riviste," in Adagio et al., *Il lungo decennio*, 265–286; Gambetta, "Alla conquista." On relations with the Christian Democratic Party, compare Umberto Gentiloni, "La Democrazia Cristiana 'primo' del sessantotto," in Adagio et al., *Il lungo decennio*, 329–344; and with the church, Cerrato, "Il mondo cattolico."

[50] Eley notes such difficulties, as in the limited success of British Labour leader Tony Benn to open up that party, and problems among Green parties, but argues that existing socialist parties—and their histories—remain indispensable to democratization as he describes it. He also speaks to historical problems of "radical democracy" from below. Political parties have most often developed from the "bottom up," while institutionalization and hierarchization have been problems beyond parties.

[51] Nationalism is a key political idea among many that has from its origins cut across the right/left spectrum. On its relation to democracy in Eastern Europe, compare Bernd Faulenbach and Heinz Timmermann, eds., *Nationalismus und Demokratie: Gesellschaftliche Modernisierung und nationale Idee in Mittel- und Osteuropa* (Essen, 1993). Compare also issues of peace and environmentalism (in formal as well as informal politics) and the role of religion.

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their minds against nuclear rearmament, inspired to join (although Wittner speaks little of it) by interests as diverse as admiration for Soviet-style governance and ultranationalism.[52] But is it clear in the end what "left" and "right" do for us here, amid other terminological questions?

It is not much of a surprise that some of these authors, such as Bruce and Kenney, generally avoid this terminology. But in the end, is that enough? Kenney's work is the most interesting in this regard, eschewing conventional characterizations of his subjects' expressed aims while lavishing universal praise for their activism. But he is careful in his phrasing, and he offers occasional interpretation of these aims. Regarding the use of Nazi images to glorify Yugoslavia, Kenney avers, "in the carnival, though, nothing was as it seemed."[53] Slovenian chauvinist nationalist chants were a "joke"; this nationalism was nothing like its Serbian counterpart. That may be true, but can we ignore the case that turned out differently? WiP's nationalism was about "peace and human rights"—and about "being in Europe." What was this Europe, and how did its past, present, and future fit together? Ukrainian activists sought alternatives to Soviet subjugation, drawing, like others, on available examples and vocabularies from the past, even if they were "looking to the future." Is it problematic that they "sought Europe" even in the precedent of nationalists in the 1940s who initially eagerly sought Nazi support?[54] Maybe not. But Gary Bruce himself includes in the category of early "building blocks of resistance" against Stalinism for "the West" the Ukrainian Insurgent Army (UPA)—best known for the "ethnic cleansing" of Poles in Ukraine. It was one thing for Hungarians in the 1980s to dig a symbolic grave for communism, but perhaps another for an East German "resister" to publicly proclaim in 1950 concerning leading communists, "God wanted [Ernst] Thälmann to be murdered in a concentration camp. [Wilhelm] Pieck and [Otto] Grotewohl will get theirs soon."[55] Was this new East German authoritarianism itself something entirely foreign, imported from outside "Europe"?

It is highly valuable to write such examples into single, difficult stories, as many of these authors do, offering overwhelming evidence of the complexity of politics, including precisely as they relate to real people's lives and experience. The question is how to comprehend it, offering corresponding complexity in the analysis. Are demands for "democracy" and for forms of national and religious purity equally meritorious? Or is it not for us to say? Is there a primacy of form over content, or of "action" over "theory," as Kenney emphasizes? One could certainly suggest that the sheer multiplicity of voices engaged in grassroots politics can help to create and reinforce forms of pluralistic inclusiveness, and the development of habits of broad

[52] Compare Belinda Davis, "The Gender of War and Peace: Rhetoric in the West German Peace Movement of the Early 1980s," *Mitteilungsblatt des Instituts für soziale Bewegungen* 32 (December 2004): 84–114.

[53] Kenney, *Carnival*, 180.

[54] Compare Konrad Jarausch and Martin Sabrow, eds., *Verletztes Gedächtnis: Erinnerungskultur und Zeitgeschichte im Konflikt* (Frankfurt, 2002). Among works by non-historians, see Vladimir Tismaneanu, *Fantasies of Salvation: Democracy, Nationalism, and Myth in Post-Communist Europe* (Princeton, N.J., 1998); Tim Judah, *The Serbs: History, Myth and the Destruction of Yugoslavia* (New Haven, Conn., 2000); Norbert Gotz and Jorg Hackman, eds., *Civil Society in the Baltic Sea Region* (New York, 2003); Miklós Haraszti, "Haiderism East of Austria," *Constellations* 7, no. 3 (2000): 305–315. Even if Kenney seeks precisely to write against some of this literature, can the issues be entirely avoided?

[55] Bruce, *Resistance*, 4, 130.

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political participation that might weather the range of content. But is there a point at which content becomes more important than form? After all, fascist activism represented a dominant example of popular politics in interwar Europe; memory of this activism was one significant factor that dissuaded Europeans from informal political expression in the postwar era.[56] Avoiding an arrogance of judgment is an achievement particularly of Kenney's work. The work of providing contextualization, alongside a disruption of assumptions and generalizations, is a central component of historians' contribution to the literature of popular politics, certainly outside as well as inside Europe. But we may need an even better understanding of what this all means with respect to postwar Europe, and for political history and explanation. And we may need some new or additional tools—terms, concepts, new ways of thinking—to reach this understanding, and to best use this history.

THIS IS IMPORTANT NOT LEAST BECAUSE SUCH RETHINKING seems central to the purposes of these historians themselves, as scholars and otherwise. Historians generally agree that their scholarship should matter for the present, however diffusely. If these authors clarify the role of the past in moving people to political engagement, they make an effective contribution to narrating this past, often in inspiring fashion. Many of the scholars under discussion here acknowledge that they are invested in their subjects, including as participant observers, and intimate their own role as activists as well as historians in writing these texts. Such investments are the source of some of these contemporary historians' most valuable resources and insights. It is important that this prospective advantage not become a liability in the story they tell, for the sake of both scholars and a broader readership. This is especially consequential in light of the emerging legions of young historians of contemporary protest. This historiography offers the possibility of sober, critical assessments, providing broad perspective and challenging mythologies. In these works, which are ambitious in both scope and purpose, there is some evidence of the difficulty of such tasks.

These authors seek to recuperate a past that in some instances has recently been deemphasized or vilified. But writing books broader both in scope and in appeal does not require that the historian's perspective be sacrificed. Many of the subjects of these texts are remarkable, often precisely in their "ordinariness," and their story well deserves telling, and fitting into a broader context. But a kind of "heroic" trope that prevails in some of these works serves both the historian and the activist questionably well in many ways. Padraic Kenney's consistently fierce enthusiasm for his subjects can leave little room for critical distance. Kenney, like some others, seems to adopt his subjects' stances unreflectively at times, for example, in assumptions regarding Western Europe and America. His proximity to his subjects, a real ad-

[56] Disaffection also related to memory of perceived "disorder"; applied also (often all the more) to communists, acting within party directives and otherwise; applied certainly to violent clashes between any groupings—and resulted for many in condemnation of governments "weak" enough to allow popular political expression generally. Naturally, the sense of "limitless possibility" and the will to make change marked fascist thinking, too. Still, this cannot be a basis for rejecting a wide range of non-institutionalized popular politics. Compare Belinda Davis, "Provokation als Emanzipation: 1968 und die Emotionen," *vorgänge* 42, no. 4 (December 2003): 41–49; Davis, "Civil Society in a New Key? Feminist and Alternative Groups in 1970s West Germany," in Karen Hagemann et al., eds., *Civil Society and Gender Justice: Historical and Comparative Perspectives* (New York, forthcoming).

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vantage in the texture of the narrative, does not make such pitfalls unavoidable; nor are these pitfalls exclusive to such work. Gary Bruce's study, cast in the Cold War template of his subjects, also betrays this heroic mode, not least in his description of the meanest "anti-government" vandalism. Such acts may well have contributed to a usable narrative of protest, but including them does not require that the actors be ennobled.[57] Indeed, such heroization detracts from the idea that assertion of the right to self-expression in postwar Europe has been in some measure the work of ordinary people on an everyday basis.

A certain Cold War stance seems to cloud Lawrence Wittner's judgment, or at least his representation of his subject. A longstanding peace activist himself, Wittner aims to encourage ongoing activism, an unimpeachable goal. But, perhaps informed by notions of what might best inspire readers, Wittner's work, remarkably readable for its length and scope, hinges on what appear to be some difficult strategies. Virtually no good communist lives in these pages. Thus the often seminal and quite substantial role of self-identified communists in peace activism across Europe is overwhelmingly disappeared.[58] Wittner may be concerned about not giving any kind of "bad press" to the movement, in contrast to that projected by evidence of the heavy involvement of church-related groups. In the event, the latter worked closely with communists, a coalition of forces key to the movement's numbers. (His characterization may also reflect the liabilities of transnational and multinational studies, above all on the scale on which he pursues such work, that is, the necessity of limited knowledge in specific contexts, and limited space to offer specificity.) In Wittner's rendering, the parties in Europe that are closest to the U.S. Republican Party come off as "hawks"; those more like Democrats are consistently "dovish," and willing to ally with popular peace demonstrators. Naturally there were tendencies. But, for example, the subtlety of reference to West German Social Democrat Helmut Schmidt's leading role in pushing NATO toward rearmament in the late 1970s, in response to the deployment of Soviet SS-20s, is notable. One convincing explanation for the alliances Wittner highlights is that parties out of power hoped that peace activists would help unseat their incumbent opponents.[59] More emphasis on these sorts of motivations might have yielded additional valuable findings concerning such alliances.

Most disconcerting in this seminal study is Wittner's fundamental assertion that there was no nuclear war because protesters convinced their leaders not to engage in it. Not only is this not proven in his book, but his own evidence challenges the case.

[57] See also Christoph Kleßmann, "Opposition und Resistenz in zwei Diktaturen in Deutschland," *Historische Zeitschrift* 262 (1996): 453–479, on the character of such acts.

[58] Leaving aside Eastern European peace protest in any way sanctioned by communist governments, and Western European activism affiliated with the World Peace Council (tied to the Soviet Union), we also see virtually nothing of the centrality of self-identified communists in, e.g., the Italian, West German, Spanish, and Dutch movements. Wittner's dismissal of communist significance in England and Austria is also problematic. Wittner, *Nuclear Abolition*, 39, 136, 335. West European communists, representing major populations in some countries, largely distanced themselves from the USSR after 1956.

[59] This indeed renders less impressive the successful pressure on Schmidt to move away from this position—in time to lose re-election. Wittner's own example, that SPD anti-rearmament activists "long suppressed . . . came surging to the fore" only once Schmidt lost to Helmut Kohl, however, questionably supports his case. Wittner, *Nuclear Abolition*, 148. The positions while in power of British Labour prime minister James Callaghan and French Socialist premier François Mitterand seem also simply represented.

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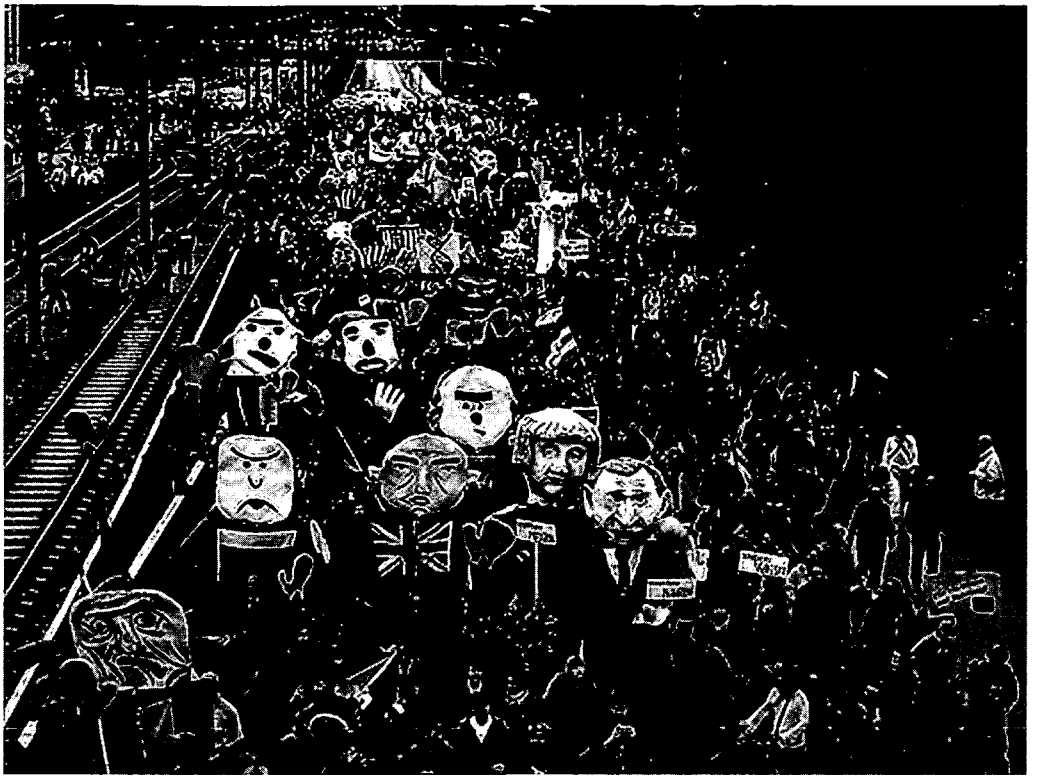


FIGURE 6: A march with puppets against the Group of Eight (G8) Summit in Rostock, Germany, in June 2007. A manifestation of the worldwide "global justice" movement, the protest in Rostock thereby exemplifies some of the characteristics that have frequently marked informal popular political activity in postwar Europe, including common protest across national boundaries and, for some demonstrators, a sense of upholding particular "European values." While the vivid scene (which was quickly marred by clashes with police) won media attention, quieter efforts to advance alternative politics continued alongside the demonstration. Image reproduced courtesy of San Francisco Bay Area Independent Media Center.

He is left to conjecture that "When most of the [government records] finally are declassified—if they ever are—they seem likely to reveal greater government responsiveness to antinuclear campaigns than government officials have admitted in their memoirs or in after-the-fact interviews."[60] The remarkably populous and inclusive peace protests of the early 1980s did not ultimately dissuade those with formal political power from supporting or at least accepting the installation of medium-range missiles on European soil, a step back from détente on both sides of the Iron Curtain. Historical representation aside, if Wittner hopes to spur popular activism with a specific political goal, he might do better to try to explain this apparent acute failure of influence. Obversely, his competing and far more nuanced claim that the longer history of peace protest yielded "big defeats" but also "small victories" is historically and politically persuasive and instructive. His useful focus on smaller acts over time offers some evidence in this regard. These sorts of forthright and judicious characterizations seem best poised to inspire as well as illuminate.

And this could be of premium value. Kenney claims that Eastern Europeans were inspired in the buildup to 1989 by "ideas about human rights, democracy, and Eu-

[60] Wittner, *Nuclear Abolition*, xi.

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ropean values."[61] These "European values" are inevitably only as good as the continued active effort to realize them. Geoff Eley observes, "Democracy in Europe has been a fragile, contested, unfinished, and relatively recent growth," one with no "natural" future and many countermanding forces.[62] These include in the present economic "precarity," struggles over ethnic and other cultural difference, and national implosions. These scholars describe a past as important to emphasize as—but not more so than—darker aspects of European history. Indeed, these are not two stories, but one very complicated one. The broader history benefits from a telling that emphasizes the agency of many "ordinary people," the agency to do things with varying appeal, and with different possible results; one that emphasizes, too, that non-action also brings outcomes. Timothy Garton Ash has expressed a sense of disquiet for the fading of a widespread vision of "Europe" that had intensified around 1989; he finds an inattention to "what story [Europe] wants to tell." Garton Ash proposes a "conversation," asking fellow Europeans to define "European values" for the present and future, and to commit to upholding them, despite the obstacles.[63] We might propose, based on this literature, that across the postwar period, "Europe" has come to tell a story, inter alia, of an overall expanding vision of political participation. But as this work also makes clear, Europeans—like all others—must continue to actively write their story a particular way, even as unprecedented heterogeneity on the continent raises hard questions about pluralism, inclusiveness, minority voices, what "Europe" is, and democracy for whom, not least in a global setting. This work also opens up for historians and others general questions concerning the nature of politics, political participation, and political change, questions well worth thinking through far beyond the borders of that continent.

[61] Kenney, *Carnival*, 10; also Hartmut Kaelble, "Demokratie und europäische Integration seit 1950," in Manfred Hildermeier et al., eds., *Europäische Zivilgesellschaft in Ost und West: Begriff, Geschichte, Chancen* (Frankfurt, 2000), 245–272.

[62] Eley, *Forging Democracy*, 3.

[63] Timothy Garton Ash, "Europe's True Stories," *Prospect* 131 (February 2007): 36–39; Garton Ash, *History of the Present: Essays, Sketches and Despatches from Europe in the 1990s* (London, 2000). Compare, however, Bella Thomas, "Reasonable Extremist," *Prospect* 132 (March 2007): 19–21, which asserts that Garton Ash, among others, sets limits on the voices that can participate. On transforming Europe, see Mazower, *Dark Continent*; among non-historians, see Daniel Levy et al., *Old Europe, New Europe, Core Europe: Transatlantic Relations after the Iraq War* (London, 2005); Ansgar Klein et al., eds., *Globalisierung, Partizipation, Protest* (Leverkusen, 2001); Donatella della Porta and Sidney Tarrow, eds., *Transnational Protest and Global Activism* (Lanham, Md., 2005).

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AHR Forum

Geoff Eley's *A Crooked Line*

Introduction

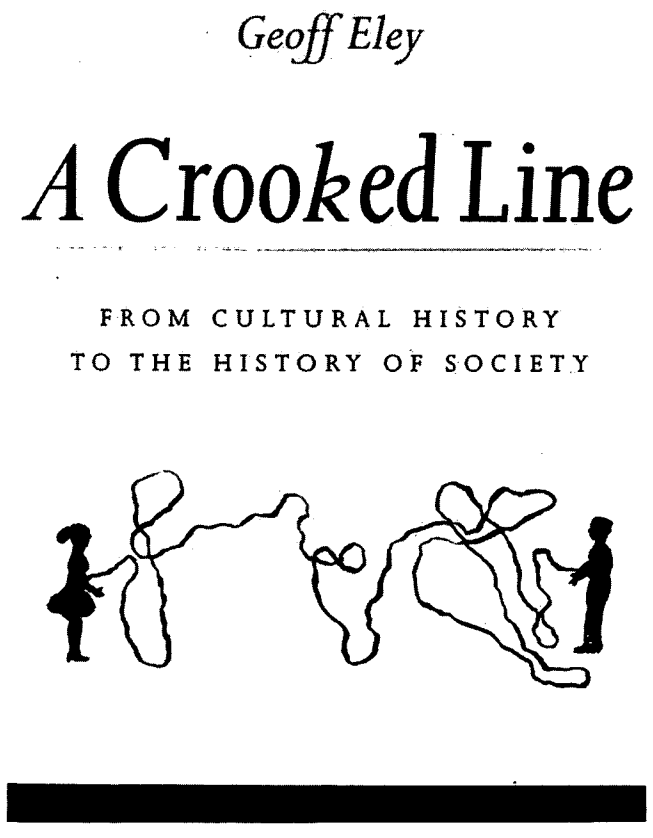
The last forty years have witnessed an extraordinary series of transformations in how historians approach the past. First insurgent, then triumphant, social history in the 1960s and '70s unleashed a remarkable outpouring of interesting studies that did nothing less than refashion our sense of the past. By the late 1980s, however, many, if not most, of its practitioners had turned to cultural history, which soon achieved hegemonic status. This simple formulation, of course, disguises the multiple methodologies, schools, and influences, both intellectual and ideological, that were part of these transformations, not to mention the variety of practices that passed for cultural history. And it is clear that the relationship between social and cultural history was more dialectical than merely sequential—that the seeds for a cultural approach were present in the critical work done by the founding figures in the social history movement. Recently, a number of historians have voiced their frustration with current methodological trends; some bemoan what has been lost in the eclipse of social by cultural history. Many, to be sure, see a source of renewal in historical thinking through approaches that are transnational or global in scope. For some, the time has come to look back in order to move forward.

Geoff Eley's book *A Crooked Line: From Cultural History to the History of Society* (2005) is a notable contribution to our understanding of how history has changed during this period. It also offers a set of arguments for how we might go beyond cultural history, without forsaking its insights, in order to recover some of the large-scale concerns that characterized social history. Finally, it is an unusual blend of the personal and historiographic: Eley uses his own biography as a means of illuminating the transformations in approaches to history, transformations that he, like many of his generation, experienced as political and moral as well as intellectual revelations.

In this *AHR* Forum, three historians with different perspectives and expertise comment on Eley's book. William Sewell, whose work has focused on France but who has also written on theory and historical methodology, criticizes Eley for underestimating the external forces—especially those related to the emergence of new forms of postwar capitalism—that, he argues, have to be understood in order to grasp the changes in our approach to history over the period. Sewell warns that an attempt to recapture the totalizing ambitions that characterized social history will require more than the historiographical "defiance" that Eley advocates. Gabrielle M. Spiegel, a

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medievalist who has also written extensively on historical methodology, calls into question Eley's account of the emergence of cultural history, pointing out some important theoretical influences coming from France that she claims he neglects. She also questions what she perceives to be Eley's call for methodological pluralism, offering instead the possibility of an actor-centered "neo-phenomenology" as a means of wedding the social to the symbolic. Manu Goswami, who has written on modern South Asia and political economy, emphasizes the deleterious cost of cultural history's triumph over social history. Most pointedly, she notes that in the context of South Asian history, it has meant both an abandonment of large-scale comparative approaches and a turn away from the concerns of political economy, thus obscuring key elements of imperialism and capitalism. In his response, Eley restates some of the key arguments in his book, explains the strategies he chose to acknowledge the contingencies, difficulties, and resistances that made up the intellectual history it covers, and responds to the criticisms of the commentators. Finally, he defends and enlarges upon his call for "new hybridities" and a "basic pluralism" in doing history today, insisting that this is neither tantamount to an abandonment of theory nor an endorsement of mere eclecticism; rather, it is an argument for "the possibility of fruitful conversation across sometimes irreducible yet mutually respectful differences."



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AHR Forum
Crooked Lines

WILLIAM H. SEWELL, JR.

GEOFF ELEY'S *A CROOKED LINE* DEFIES CLASSIFICATION in existing genres of historical writing. It partakes of intellectual history, memoir, theoretical treatise, and political tract without being quite any of these; it embodies the sort of experiment with form that Eley praises in the work of Carolyn Steedman, one of his historiographical heroines. Its deepest affinity seems to be with the work of the British Marxist literary critic Raymond Williams, whose name and example emerge again and again at crucial points in the text. Eley's argumentation and in fact his very sentences have the slow, complex, dialectical, and ruminative quality that one has learned to associate with Williams—he is never quick to judge, always aware of the many-sidedness of the problem at hand, theoretically alert but wary of abstraction, ever reflective about his own historical placement within the argumentative complex, yet firm and consistent in his Marxist-humanist moral and political bearing. Eley's book displays an admirable realism and humility in the face of the many surprises and disappointments that have troubled the historical experience of the "sixties generation" and a willingness to learn both from that experience and from other people whose judgments and perspectives are different from his own.

One of Eley's purposes is to show what it means to be both intellectually ambitious and politically engaged as a historian. "My hope," he writes, "is that mapping a series of personal encounters between the tasks of historical writing and the surrounding political climate may make it possible for others to recognize their own analogous accounts, whether converging with mine or not" (6). Eley makes it clear that the problems of historical engagement reach to the core of the historian's emotional being: the chapters of the book, after the initial "Becoming a Historian," have titles that evoke changes in Eley's dominant feelings about history and politics over time: "Optimism," "Disappointment," "Reflectiveness," and "Defiance." Eley says he became a historian "because history really *mattered*; it was necessary for making a difference" (ix). He has sustained this motivation through all the twists and turns (a crooked line, as he puts it) of both world politics and professional historiography during the course of his career. The book is, among other things, an eloquent testimony to history as a moral calling; students contemplating a career in our discipline should read it and take it to heart.

The chief subject matter of the book is the two great historiographical movements that have reshaped the profession since Eley began his undergraduate work at Balliol College, Oxford, in 1967: the rise of social history in the 1960s and 1970s and the

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turn to cultural history in the course of the 1980s and 1990s. Eley's account of these transformations is written from the standpoint of his own career and his own engagements as a historian and is, hence, biased toward European history, particularly German and British, with an emphasis on history written from a left-of-center perspective. (His extended discussion of the rise and triumph of social history in Germany, and its eventual challenge by *Alltagsgeschichte*, is particularly masterful.) The range of reading and historiographical reference is, however, very wide, and the book includes shrewd observations on American, French, and South Asian historiography. Nor do other disciplines escape his gaze: Eley comments on developments in sociology, anthropology, cultural studies, gender studies, postcolonial studies, and literary criticism, all of which, as he notes, have played crucial roles in the transformation of historical writing and research, including his own, over the past four decades. His footnotes, which take up nearly a third of the book, are a goldmine of references and commentary.

Eley was himself a participant in both of the great historiographical movements he narrates. Entering graduate school (at the University of Sussex) in 1971, he was part of the generation that rode the rising tide of social history. In his English milieu, social history was fundamentally Marxist in orientation—strongly influenced by E. P. Thompson, Eric Hobsbawm, George Rudé, Christopher Hill, and other British Marxist historians. However, as Eley points out, the other major schools of social history—the French *Annales* school and international but especially American social science history—had highly convergent programs. All were in search of what Charles Tilly pithily termed *Big Structures, Large Processes, Huge Comparisons*; all had the ambition of grasping historical totality; all were open to interdisciplinary collaboration with other social sciences; and all were committed to an essentially materialist model of explanation in which economic structures held pride of place.[1] Rather than rivals, the three styles of social history seemed collaborators on a common project.

But social history had no sooner gained hegemony in the historical profession (toward the end of the 1970s) than some of its leading practitioners began to assert the insufficiency of materialist explanation and to emphasize instead the importance of cultural structures and processes. Over the course of the 1980s and accelerating into the 1990s, what came to be called cultural history overtook social history as the leading edge of scholarship—but not without some fierce struggles. Like the social history movement, this linguistic or cultural turn in history was international in scope and profoundly interdisciplinary. But the disciplinary partners shifted from sociology, political science, geography, and economics to anthropology, philosophy, cultural studies, and literary criticism. Just as the social history movement was deeply influenced by populist or socialist political aspirations, the cultural history movement was pushed forward by a new set of political passions—by feminism, most decisively, but also by identity movements around race and ethnicity. At the same time, the declining plausibility of totalizing socialist utopias converged with a new concern for the local and for questions of personal identity. The search for big structures gave way to microhistories and histories of subjectivity; meanwhile the certainties of structural explanatory strategies were corroded by the poststructuralisms of Foucault,

[1] Charles Tilly, *Big Structures, Large Processes, Huge Comparisons* (New York, 1984).

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Derrida, and Lacan and by the vogue for postmodernism. Eley, while never a prime advocate of these changes, was certainly sympathetic to the new sensibility of cultural history; at the same time, he never renounced the perspectives of social history.[2] Now, at the beginning of the twenty-first century, when the novelty of cultural history and the linguistic turn has begun to wear off, Eley ends his book with a plea for theoretical and methodological pluralism: "between social and cultural history," he concludes, "there is no need to choose" (181).[3]

The book stimulates two lines of rumination. First, as Eley hoped it would, his story has prompted me to think about how my historiographical and political engagements resembled and differed from his. His account is, at one level, immediately recognizable to any historian who has lived through these years. But it is also very particular. I, for one, was struck by how Anglo-centric Eley's attachments and judgments continue to be, after his nearly thirty years at the University of Michigan. The three superb intellectual portraits of historians that he uses to conclude his central chapters—Edward Thompson for "Optimism," Tim Mason for "Disappointment," and Carolyn Steedman for "Reflectiveness"—are all English, as is Raymond Williams, Eley's most consistent point of theoretical reference, and Eric Hobsbawm, whose "History of Society" slogan is one of Eley's organizing tropes. As an American-born historian who has struggled through the same general succession of historiographical changes, I find that the specificity of this English standpoint serves to highlight specificities of my American experience. Comparing the crooked lines marked out by these two trajectories poses questions about how we should understand the relationship between historical context and historiographical changes over the past four decades.

My second rumination, apparently unrelated but in fact closely linked to the first, arises from a sense of dissatisfaction with Eley's stance in the historiographical present, particularly in his final chapter, "Defiance." It is, of course, difficult to know quite how to judge or to criticize a book so utterly *sui generis* as this one. As a report on Eley's own ambivalent feelings about contemporary politics and historiography, his concluding chapter is as eloquent as those that precede it. Yet I find his claim of defiance out of step with his seemingly uncritical stance toward the currently dominant style of historiographical practice, what is commonly labeled "the new cultural history." I believe that leftist historians can be productively defiant in the dispiriting political conjuncture of the early twenty-first century, but that it will require some hard theoretical labor on our part. We need both to find a theoretically satisfactory way of overcoming the current divide between social and cultural history and to recognize how our own efforts to rethink history are conditioned by contemporary transformations in global capitalism.

THE CROOKED LINE OF MY OWN HISTORIOGRAPHICAL DEVELOPMENT broadly parallels Eley's. It was, moreover, tightly intertwined with his for a while. When I taught at

[2] His somewhat ambivalent position was well stated in "Is All the World a Text? From Social History to the History of Society Two Decades Later," in Terrence McDonald, ed., *The Historic Turn in the Human Sciences* (Ann Arbor, Mich., 1996), 193–243.

[3] The point is repeated on p. 201.

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the University of Michigan in the later 1980s, Geoff Eley and I were both members of the steering committee of the Program on the Comparative Study of Social Transformation. CSST, as it was called, was an explicitly interdisciplinary program including historians, anthropologists, sociologists, political scientists, and literary scholars. It was the prime site of the discussions (and hammer-and-tong intellectual battles) that accompanied the linguistic or cultural turn in Ann Arbor in those years—and a perfect example of the sort of dialogue between disciplines and theoretical perspectives that Eley properly identifies as a potent source of historiographical innovation. During the heady days of our joint involvement in CSST, our historical lines tracked each other's very closely.

But if we generally saw eye to eye in those years, our previous historiographical and political development had been quite different. Although I am ten years older than Eley, I, too, was deeply influenced by the politics of the 1960s. I grew up in Madison, Wisconsin, the son of New-Dealer parents. For me, coming of age in the 1950s, the burning issue was not the workers' struggles but civil liberties and civil rights. The first political cause I strongly identified with was the "Joe Must Go" campaign—an unsuccessful attempt in 1954 (when I was fourteen) to recall Joseph McCarthy, my state's junior senator. During my college years at the University of Wisconsin, the dominant issue was the civil rights movement. Even during my graduate school years at Berkeley, it was a kind of radical left-liberalism rather than Marxism that defined the political terrain. The free speech movement of 1964, defending the right of students to organize on campus for civil rights activism, was the great galvanizing event. Issues other than free speech and civil rights of course arose in ensuing years—most importantly, opposition to the War in Vietnam. The Bay Area was also at the center of "lifestyle" cultural radicalism in the 1960s: experimentation with psychedelic drugs, the San Francisco rock music scene, the sexual revolution, gay liberation, communes, challenges to conventional sex roles—the entire panoply of what we called "the expansion of consciousness." In Berkeley in the 1960s, the venerable liberal right to the pursuit of happiness was given a powerful new impetus. By contrast, Marxism, while present in the vibrant politics of Berkeley, was hardly central.

I arrived at Berkeley determined to study social history. My outlook partook of the default economic determinism of the moment, yet I was not a Marxist. I read and was thrilled by E. P. Thompson's *The Making of the English Working Class* in the winter of 1964, when I was developing my dissertation project in labor history. But what impressed me most in the book was Thompson's depiction of the enormous diversity in circumstances and attitudes *within* the laboring population, and especially his demonstration that artisans took the leading role in the workers' struggles. For me, Thompson's book served as a welcome blow *against* the then-conventional Marxist argument in labor history—that worker radicalism resulted from the growth of a uniform factory proletariat. Thompson's findings seemed to me consonant with detailed quantitative studies of the relationship between social structure and political engagements by sociologically inspired scholars such as Stephan Thernstrom and Charles Tilly.[4] In European, and specifically French, labor history, Marxism

[4] Stephan Thernstrom, *Poverty and Progress: Social Mobility in a Nineteenth-Century City* (Cambridge, Mass., 1964); Charles Tilly, *The Vendée* (Cambridge, Mass., 1964).

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seemed the dead hand of tradition, not the cutting edge. By contrast, liberal American social science offered valuable tools for getting at the social origins of nineteenth-century worker radicalism.[5]

My lack of political or intellectual commitment to Marxism undoubtedly made my subsequent move to cultural history easier. Although my common sense was materialist, my materialism lacked political weight. When my University of Chicago colleagues Bernard Cohn and Ronald Inden introduced me to the wonders of symbolic anthropology in the early 1970s, I had no political reason to resist. Besides, I was growing frustrated with the limitations of quantitative social history, which could indeed reconstruct social structures in exquisite detail, but which remained silent about what people felt or believed. Approaches borrowed from anthropology were attractive because they promised to yield deep insights into the meaning of workers' utterances and collective action. But for me, this desire to discover people's cultural meanings also had a charge that went beyond the purely intellectual. I had been vaguely aware ever since graduate school that the quantitative techniques definitive of social history had a complicated political valence. On the one hand, they had a certain democratic implication because they seemed the best means of getting at the experiences of the sort of people—for example, workers and peasants—who rarely left behind written sources. But on the other hand, quantification was redolent of precisely the bureaucratic corporate mentality that the counterculture of the 1960s found deeply objectionable. Taking on the intellectual perspective of cultural anthropology not only produced new data about workers: it seemed positively liberating. Rather than being limited by a narrow and bureaucratic form of research that gave me access only to the outward forms of working-class life, I could now explore the surprising and wonderfully variegated realms of workers' symbolic life—effectively, their consciousness and collective unconscious. These new forms of research revealed alternate moral universes by no means reducible to the facts of occupational structure, wage rates, and degrees of economic exploitation. For me, and I think for many other historians of my generation, cultural history resonated with the yearnings of the 1960s counterculture. I certainly never renounced quantitative history, but from the mid-1970s forward I began to regard quantification more as an ancillary technique than as the royal road to historical truth.[6]

Up to this point in my career, I had been rather dismissive of Marxism as a frame-

[5] That these tools were indeed valuable was demonstrated by several young American scholars of French labor history, including Robert Bezucha, *The Lyon Uprising of 1834: Social and Political Conflict in the Early July Monarchy* (Cambridge, Mass., 1974), and Joan Wallach Scott, *The Glassworkers of Carmaux: French Craftsmen and Political Action in a Nineteenth-Century City* (Cambridge, Mass., 1974). My dissertation on the working class of nineteenth-century Marseille was never published, but for my work in this vein, see William H. Sewell, Jr., "The Working Class of Marseille under the Second Republic: Social Structure and Political Behavior," in Peter N. Stearns and Daniel J. Walkowitz, eds., *Workers in the Industrial Revolution: Recent Studies of Labor in the United States and Europe* (New Brunswick, N.J., 1974), 75–115, and "Social Change and the Rise of Working-Class Politics in Nineteenth Century Marseille," *Past and Present* 65 (November 1974): 75–109.

[6] *Work and Revolution in France: The Language of Labor from the Old Regime to 1848* (Cambridge, 1980) was the principal product of this period of my historiographical development. Its arguments cut strongly against the predominant Marxisant themes of labor history in this era. French socialism, it argued, was a product not simply of the industrial revolution or even of "proletarianization," but also of a political and cultural struggle that created a new working-class political culture between 1830 and 1848 by grafting old regime guild solidarities onto the language and political forms of the French Revolution.

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work for social history, on the grounds that it tended so strongly toward reductionism.[7] But during the late 1970s and the 1980s, I began slowly to revise this position. Getting to know the Marxist economists David Gordon, Herb Gintis, and Rick Edwards and the Marxist art historian Tim Clark during my years at the Institute for Advanced Study in the late 1970s and participating in a Marxist study group at the University of Arizona in the 1980s made it clear that my grasp of Marxism had been utterly superficial and that I had a lot to learn. So by the time my crooked line began to overlap with Eley's in Ann Arbor in the later 1980s, my appreciation of Marxism had risen considerably. At the same time, political developments in the 1980s, particularly Reaganism in the United States and Thatcherism in Britain, made it clear that class war, of the rich against the poor, was alive and well. The wrenching changes in the U.S. economy—massive deindustrialization, the shift of power from the Northeast and Midwest to the South and West, the globalization of finance—made issues of economic structure and change seem absolutely urgent. Finally, the fall of the Berlin Wall, which was seen by many commentators as marking the definitive end of Marxism, seemed to me to liberate Marx from the grotesque totalitarian regime that had falsely claimed to incarnate his ideas. Thus, during the time we worked in tandem at the University of Michigan, Eley and I were both eclectics, drawn simultaneously by exciting innovations in cultural theory and an appreciation of Marxist perspectives. And since our formal partnership ended in 1990, when I moved back to the University of Chicago, both of us have continued to look for ways simultaneously to embrace social history, Marxism, and cultural history. Both Eley and I have taken defiant political postures in the face of the left's political difficulties since 1980, refusing to accept the seeming triumph of global capitalism and plutocratic democracy as anything like the "end of history." For the time being, at least, our lines continue to advance, however crookedly, in roughly parallel directions.

ANY READER OF ELEY WILL RECOGNIZE THIS THUMBNAIL SKETCH of my historiographical development as in some sense a variant of his. The differences in our trajectories—for example, Eley's early commitment to Marxism and my early liberalism or my more precocious and emphatic linguistic turn—are easy to explain by our particular biographies. But what about the similarities, which clearly were shared by many historians in our broad generation, not only in North America and Europe, but in South Asia as well?[8] In all of these places, there was an upsurge of social history in the 1960s and 1970s, followed by a move to cultural history in the 1980s and 1990s. The timing and the particular details varied, of course, but the sequence has been surprisingly uniform.

Although Eley discusses a wide range of national historiographies, he rarely steps back to consider the reasons for this wider transnational pattern. It is paradoxical that while he consistently advocates retaining social history's totalizing ambition, he

[7] I had always admired the work of E. P. Thompson and Eric Hobsbawm, but thought of them as unrepresentative mavericks.

[8] For these purposes, I would define our generation as roughly those born between the late 1930s and the early 1950s. I am too ignorant of the historiography of other areas of the world to know whether the same pattern applies to historians in, say, Africa, East Asia, Latin America, or the Middle East.

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fails to pursue this ambition in his own historical account of recent historiography. He gives us a fine-grained analysis of intellectual movements in the discipline and relates these to ongoing political struggles, but fails to relate them to the development of global capitalism—which, for him as a Marxist, would seem to have defined, or at least participated importantly in defining, the social totality within which both historians and political actors were constrained to act.

About the only general claim that Eley makes about the initial rise of social history is that "the radical politics of the sixties were inseparable from the historiographical story. The breakthrough to social history was unimaginable without the sense of political possibility beckoning during the later 1960s" (59). The radical political movements called up by the metonym "1968" were, of course, famously international, and this quasi-global radicalism certainly fired the imaginations of many social historians in the making. But in my opinion, 1968 is better understood as giving a particular spin to a historiographical movement that was already well under way. When I arrived in Berkeley in 1962, many graduate students, myself included, were already planning to work in social history; by 1968, we were well into our dissertations. Instead of seeing the political radicalism of the late 1960s as a necessary *antecedent* to the "breakthrough to social history," I think this breakthrough and the ubiquitous radical movements of these years must *both* be seen as arising from deeper social causes. Elaborating a full argument to this effect would take much more space than I have here. But I think that the epistemological optimism of social history—its faith in the possibility of reconstructing a history of the social totality—was made plausible in large part by the specific form of capitalist development that characterized the great worldwide postwar capitalist boom. So-called "Fordist" or state-centered capitalism, with its fundamental pact between big business, big labor, and big government, its standardized mass production, its Keynesian steering of the economy, its fixed exchange rates, and its global guarantee by United States military power, had produced, or at least seemed to have produced, a graspable, predictable, and steadily progressing form of society. To use Raymond Williams's language, one might say that the "structure of experience" generated by postwar capitalism underwrote the plausibility of social history, whether in its Marxist, its *Annaliste*, or its social-scientific form.[9]

But so, in a different but related way, did Fordist capitalism underwrite the 1960s revolts. Those revolts were spearheaded by young people, and more specifically by university students. As Daniel Bell pointed out in *The Coming of Post-Industrial Society*, the type of capitalism that arose in the postwar boom in the wealthy countries was increasingly dependent on the production and management of knowledge.[10] This required a better-trained labor force, which entailed a vast expansion of the university systems in all advanced democracies. In the late 1950s and the 1960s, university students made up a far higher proportion of their age cohorts than ever before. They were generally ensured of good jobs once they graduated, were con-

[9] For a similar argument about sociology in the 1950s and 1960s, see George Steinmetz, "Scientific Authority and the Transition to Post-Fordism: The Plausibility of Positivism in American Sociology since 1945," in Steinmetz, ed., *The Politics of Method in the Human Sciences: Positivism and Its Epistemological Others* (Durham, N.C., 2005), 275–323.

[10] Daniel Bell, *The Coming of Post-Industrial Society: A Venture in Social Forecasting* (New York, 1973).

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fident about the future, were living independently without adult responsibilities, and were supplied with inexpensive paperbacks and new and effective birth-control technologies. Although university students were clearly beneficiaries of the Fordist boom, universities also afforded them the social space and intellectual resources to develop a critical political culture and to experiment with new lifestyles. The student milieu combined the optimism that grew out of the apparently permanent prosperity of the postwar boom with a highly critical attitude toward the form of capitalism that in fact enabled that prosperity. The student radicals' rhetoric and their mode of life was quite specifically anti-Fordist—especially hostile to bureaucracy, corporate conformity, and mass culture. It seems fair to conclude that the student movements of the 1960s were deeply embedded in the contradictions of Fordist capitalism, at once dependent on its promises of endless prosperity and insistently pointing beyond it to a less stultifying form of life that its very material abundance made thinkable. In short, understanding the rise of either social history or the radical movements of the 1960s—both of them clearly transnational phenomena—can be enhanced by showing how these phenomena were linked to the broad forms and dynamics of global capitalism in their eras.

I think Eley's account of the cultural turn suffers from the same sort of problem as his account of the initial rise of social history. Again his story combines intellectual and political history via personal narrative, but it contains little reflection about the macrosocial environment in which the historiographical changes took place. In the course of his account, Eley mentions two aspects of the sociopolitical environment of the late 1970s and 1980s that undermined social history's modes of explanation. On the one hand, "class was weakening in its persuasiveness as a master concept" (100), largely because of the decline of the labor movement internationally after about 1976 and the shocking victory, with significant working-class support, of a viciously anti-labor Thatcherist Conservative Party in Britain in 1979 and again in 1983.[11] The other was the rise of feminism, a political and intellectual movement that no self-respecting leftist could ignore, but that, by the 1980s, was insisting that "fundamental areas of social life simply couldn't be subsumed into the analytical terms of class" (98). For a British social historian whose view of the social totality was premised on class, this was indeed a potent combination. But here Eley's account is too local to make sense of the overall historiographical and political conjuncture. The weakening of the labor movement certainly was discouraging for the left everywhere, and feminism everywhere posed conundrums for the implicitly masculinist assumptions of social history and leftist politics. But for social historians not strongly cathected to class as an analytical category—for example, American social-science historians like me or French *Annalistes*—the fading plausibility of class explanation was a matter of relative indifference and therefore cannot explain their turn to cultural history.

In my opinion, the decline no less than the rise of social history must be connected to the changing macrosocial forms and fortunes of world capitalism. The postwar boom that underwrote both the rise of social history and the political-cultural revolts of the 1960s ceased abruptly in the early 1970s, and the world economy entered a

[11] The equivalent American events were Ronald Reagan's victories in 1980 and 1984.

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period of sustained structural crisis. It was not just that economic growth slowed, but that the underlying structures of Fordist capitalism came unraveled over the course of the 1970s and 1980s. Industrial heartlands became "rust belts." Keynesianism, which could not solve the riddle of "stagflation," gave way to monetarism and microeconomics. The system of pegged exchange rates collapsed, making way for the hypertrophic growth of "off-shore" currency speculation, enhanced, of course, by the new electronic communications technologies. Financial services replaced manufacturing as the leading sector in the wealthiest countries. Labor unions declined in power and numbers. Corporations themselves metamorphosed from hierarchically structured "national champions" to much more loosely structured "multinationals" that, taking advantage of the new electronic communications technology, could locate manufacturing, back-office work, and technical services wherever they could be performed most cheaply. At all levels of the occupational hierarchy, both for managers and for production workers, job security and the well-defined career ladder were eroded, as workers increasingly experienced a kind of occupational picaresque, featuring lateral movement between firms, bouts of temporary work, self-employment, and frequent retraining. International trade surged, as did international labor migration, both legal and illegal. Nation-states were ever less capable of controlling the economic activities that went on within their boundaries; indeed, some have claimed that the very notion of a "national economy" ceased to make sense. The state-centered sociopolitical imaginary of the postwar era, with its confidence in state economic steering, growing welfare-state benefits, guarantees of full employment, and state-led cooperation between labor and capital, lost its hold. This older political imaginary was gradually and unevenly displaced by a rising "neoliberal" political imaginary, one that exalted individual responsibility, universal entrepreneurship, privatization, deregulation, and globalization.

It is puzzling that an avowed Marxist such as Eley never asks how the epistemic practices of historians might have been affected by such a fundamental transformation of the social forms of world capitalism. Most obviously, if the consolidation of Fordism in the 1950s and 1960s made social structures seem graspable, predictable, and quantifiable, it makes sense that the unraveling of Fordism in the 1970s and 1980s undermined the plausibility of the social history paradigm. By the later 1970s, not only politics and social structure but even personal identities seemed disconcertingly up for grabs. When historians turned from the pursuit of big structures to microhistory, from socioeconomic determinism to studies of culture, from master processes to the sources of subjective identity, this search for new forms of intelligibility in the past reflected the difficulties of making sense of a present in which Fordist capitalism had disintegrated and was being replaced by the more fluid and unpredictable forms of an emerging global neoliberalism.[12] Nor was it only historians who, in these years, eschewed the reified categories of the previous intellectual era. Anthropologists abandoned the conventions of preexisting ethnography;

[12] The French historian Jacques Revel would seem to agree. Concerning the late 1970s and the 1980s, Revel remarked that "the doubts that . . . spread through our societies, confronted as they were by forms of crises that they could not comprehend, nor even, in many cases, describe, has certainly contributed to the diffusion of a conviction that the project of an overall intelligibility of the social had to be, at least provisionally, put in brackets." Revel, "Microanalyse et construction du social," in Revel, *Jeux d'échelles: La micro-analyse à l'expérience* (Paris, 1996), 18.

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literary critics adopted deconstruction; postmodernism became a rallying cry across a whole range of academic disciplines. Even social scientists who continued to embrace mathematics and scientific method reflected this tendency to eschew reified structures and rebuild their disciplines from the ground up. Rational choice, with its stringent assumption of methodological individualism, made great strides in economics and political science. And in sociology, the new methodology of social network analysis insisted that social structures could not be simply posited, but had to be built up laboriously from the social interactions out of which they were ultimately constituted. The era of transition from Fordist or state-centered capitalism to the globalized capitalism of neoliberalism was characterized all across the human sciences by a general epistemic uncertainty—an uncertainty that has a certain elective affinity with the heightened "flexibility" that is one of the hallmarks of the new global economic order.[13] In history, this uncertainty took the form of the cultural turn, flirtations with poststructuralism, and a fascination with microhistory and subjectivity.

In the 1980s and the 1990s, many historians were intoxicated by the sense that the linguistic turn had liberated them from the stifling frameworks and static politics of social history. But that mood seemed to change as global neoliberalism consolidated itself around the turn of the new millennium. The spectacle of soaring executive salaries combined with stagnant wages, the increasingly apparent erosion of democracy by plutocracy, and the unambiguous exaltation of exchange value over all other forms of value have inspired a kind of nostalgia for social history—which, for all its failings, at least attempted to wrestle with the problem of large-scale socioeconomic transformations. Eley, with his current mood of political defiance and his affirmation of the continuing value of social history, is a prominent example. He might be a bellwether of a coming historiographical trend; it is too early to know.

ELEY'S SLOGAN FOR THE CURRENT ERA OF "DEFIANCE" is that "between social history and cultural history, there is really no need to choose" (181). His basic desire, if I understand him properly, is entirely laudable: to revive social history's effort to grasp (capitalist) social totality without giving up the immense intellectual gains made possible by the cultural turn. But if the goal is laudable, I do not find Eley's final chapter very effective at pointing the way—above all, in my opinion, because he has not found a theoretical perspective adequate to the task.

At one level, Eley's claim that there is no need to choose turns out to be little more than an expression of satisfaction with the sort of work being done at present under the banner of "the new cultural history." In this formulation, the admonition to retain social history is weak, since it is far from clear whether anything worthy of the name of social history is currently on offer. Thus Eley declares at one point that social history "simply isn't available any more," that it has "ceased to exist" as a coherent project, and that each element of its paradigm "has succumbed to relentless and compelling critique" (189). Nothing remains, Eley implies, but fragmentary

[13] On "flexible accumulation," see David Harvey, *The Condition of Postmodernity: An Enquiry into the Origins of Cultural Change* (Oxford, 1989).

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themes or topics drifting free of the old social history paradigm. Near the very end of the book, he argues that by the late 1990s, "the new cultural history," now the "broadly accepted description" for the best work going on in the field, had actually become an "eclectic repertoire of approaches and themes" whose boundary with social history was "extraordinarily more blurred." Here Eley celebrates younger historians' ability to muddle through, to find, under the banner of the new cultural history, concrete ways of combining social and cultural themes and topics while avoiding "the programmatic advocacy of one authorizing form of theory against another." He praises the "hybridity" of the "new cultural history" for enabling historians to set theory aside and get on with a wide range of interesting empirical work (201).

But these rather uncritical paragraphs are followed by another that points in a quite different direction. "Some confidence," Eley writes, "needs to be regained in the possibility of grasping society as a whole, of theorizing its bases of cohesion and instability, and of analyzing its forms of motion" (201–202). Here Eley indicates, I believe, his most deep-seated reason for continuing to advocate a combination of social and cultural history: his appreciation of social history's sense of the social totality. In one mood, he seems content that various themes and topics recognizably derived from the now-shattered social history paradigm can find their place in the shapeless bazaar of the new cultural history. But in another mood, he is discontented that historians have abandoned their efforts to grasp social totality. Yet even then he remains diffident, as indicated by the far from ringing plea that "*some* confidence needs to be regained in the *possibility* of grasping society as a whole." He goes on to say that "neither skepticism about the persuasiveness of grand narratives nor critiques of Enlightenment thinking require *altogether abandoning* the project of societywide analysis or societal history" (202; my emphasis). He then admits that "for my own part, I've continued thinking in terms of capitalism, class, the nation, social formation, and so forth." He has not, in other words, "altogether abandoned" his Marxist categories. "But," he continues, "I am far more cautious and uncertain about exactly what these grand-theoretical concepts can allow me to discuss and explain." The paragraph that began with a call to revive social history's effort to grasp social totality ends not with an affirmation of the confidence he says must be restored, but with reflections on the historical contingency of the very concepts, such as class and society, by which totality might be grasped (202).[14]

Yet in the book's final paragraph, Eley puts the ambivalence behind him to suggest "defiance" as "the appropriate response for our new contemporary moment." In an era beset by grand narratives of neoliberalism and "brutally demonizing rhetorics about good and evil in the world," he suggests that we (by which he means leftist historians) need to develop metanarratives of our own, "new 'histories of society' " (203). I entirely agree with this conclusion. But I think that defiance must be more than an attitude, and that any attempt at writing new histories of society (with the totalizing ambitions Eley intends) must face head-on some theoretical difficulties that Eley avoids in this book.

I see two fundamental theoretical problems. First, it is necessary to work out *in*

[14] Here, as so often in this book, one must admire Eley's willingness to articulate, openly and painfully, his intellectual and political ambivalence.

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theoretical terms some means of combining, on the same epistemological terrain, the materialism of "social history" and the idealism of "cultural history." In a recent book, I have offered my own attempt at such a theoretical reconceptualization. I begin by denying that all social relations are reducible to language, but argue that because all social relations have a meaningful content, they can nevertheless be grasped by a modified or expanded version of the linguistic model. I try to show that the whole range of human behaviors—for example, such activities as work, sex, cooking, currency speculation, or basketball—can profitably be understood as constituted by a web of "semiotic practices." I also argue that if the implications of such an approach are followed out correctly, we will find that interconnected semiotic practices accumulate into what I call "built environments"—materially existing and spatially located physical and social fabrics that endure but are also transformed by continuing flows of semiotic practice. This theoretical framework may or may not seem promising, but it explicitly goes beyond a mere attitude of defiance to meld social and cultural history into a conceptually coherent and unified historiographical project.[15]

The second necessary theoretical task is to rethink the problem of social totality. The striking and often brutal transformations of capitalist social relations since the 1970s have helped convince me that capitalism is the crucial horizon of social totality, not only in the present, but in the entire modern era. From this it follows that rethinking social totality requires an engagement with Marxism, since, in my opinion, it is adherents of this tradition who have thought most deeply and productively about capitalism. My own predilections within Marxist debate differ, I believe, from Eley's. I read his remarks on Marxism in *A Crooked Line* to imply that for him, class is the fundamental category of Marxist analysis. I would tend to emphasize the endless accumulation of capital as forming the crucial underlying dynamic of capitalism, with class and class struggle figuring more as a context and outcome of the dynamic of accumulation. In capital-centered Marxist theories, the endless accumulation of capital produces changing historical configurations of political power, spatial relations, class struggles, intellectual forms, technology, and systems of economic regulation that endure for a certain time until they are dismantled by their own contradictions and replaced by new configurations.[16] As I see it, these reconfigurations of capitalism are as much cultural as material processes—evolving sets of semiotic practices and at the same time evolving "built environments." The difference between these two conceptions of capitalism is, I believe, consequential for rethinking social totality. It was, after all, the class-centric model of social totality that withered under the assaults of neoliberal capitalist restructuring and feminist theory in the late 1970s and 1980s; I think that conceptions of social totality centered on endless accumulation (such as the one I have attempted, however crudely, to sketch out above in

[15] This account is compressed, perhaps to an absurd degree, from chapter 10, "Reconfiguring the 'Social' in Social Science: An Interpretivist Manifesto," in William H. Sewell, Jr., *Logics of History: Social Theory and Social Transformation* (Chicago, 2005), 318–372.

[16] For three very different historical perspectives on the endless accumulation of capital, see David Harvey, *The Limits to Capital* (Oxford, 1982); Giovanni Arrighi, *The Long Twentieth Century: Money, Power, and the Origins of Our Times* (London, 1994); and Moishe Postone, *Time, Labor, and Social Domination: A Reinterpretation of Marx's Critical Theory* (Cambridge, 1993) and "Contemporary Historical Transformations: Beyond Post-Industrial Theory and Neo-Marxism," *Current Perspectives in Social Theory* 19 (1999): 3–53.

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my account of the global capitalist transformations following the 1970s) will prove much less vulnerable.

Geoff Eley's *A Crooked Line* is a powerful stimulus to reflection on the political entailments and theoretical challenges of history—both as written and as lived. It goes without saying that not everyone will agree with his judgments. As I have indicated, I think that finding a way forward from the current historiographical perplexity will require a stronger dose of theory and a different sort of theory than Eley has offered. But he has masterfully sketched out the terrain on which the debates must take place.

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AHR Forum
Comment on *A Crooked Line*

GABRIELLE M. SPIEGEL

IN *A CROOKED LINE*, GEOFF ELEY OFFERS a survey of the fundamental transformation that overtook the practice of social history between the 1970s and 1980s with the rise of cultural history in response to challenges to prevailing forms of social history posed by the linguistic turn. Triangulating, as he says, "the political, the historical and the personal," the story he tells is embedded in the context of his own formation as a historian, a formation with a distinctively British (and Marxist) voice, and one surprisingly free of French accents, Michel Foucault excepted.[1] It is a story, as well, of a particular professional generation that came to maturity in the aftermath of the 1960s and was deeply committed to historical change in the present, a problem that its members approached both in their lives and in their work, and through the interpenetrating influences that this conjuncture implied. One of Eley's goals in *A Crooked Line* is to map "a series of personal encounters between the tasks of historical writing and the surrounding political climate" (6) in order to underline the inevitable imbrication of history and politics in any historian's thought and writing—in short, to advocate a historiographical practice "powered by the politics of commitment and the ethics of conviction" (7). It is worth noting that the chapter headings by which Eley denotes his participation in the changing styles and aims of historiography over the last four decades point to the psychic and emotional investments that accompany the historian's labor: "Optimism," "Disappointment," "Reflectiveness," and "Defiance."[2]

I would like to thank my colleague David Nirenberg for his comments on this article, from which, as always, I have benefited greatly.

[1] In terms of the form of this quasi-autobiographical account, it falls somewhere between what Jaume Aurell has recently called "constructionist" autobiography and "experimental" autobiography. The first is defined as autobiography in which authors tend "to establish a critical distance from their own lives to present it objectively, often in terms of empirical-analytical language that gives their narratives a monographic air." "Experimental autobiographers," on the other hand, are "less concerned with their identity as academics/historians, and narrate with an epistemologically skeptical frame," although they are concerned to specify and identify with "specific intellectual itineraries, [in which] history [becomes] a subtext of their personal narratives." See Aurell, "Autobiography as Unconventional History: Constructing the Author," *Rethinking History* 10, no. 3 (2006): 433–449, esp. 435 and 439.

[2] The first two chapters are devoted to a review of his training as a historian in Britain and Germany, primarily under the impulse of and inspired by the rise of the "new" social history of the 1960s and 1970s. They offer the non-British, non-German historian an extremely interesting and valuable review of the major trends in social history in those countries during those two decades, that is, before the advent of the linguistic turn. The last two chapters take up the question of the diverse relations between social and cultural history, the emergence of linguistic turn historiography, the ways in which it affected the practice and theory of history, and finally a "defiant" consideration of the potential for a revised, more

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Moreover, it was Eley's generation—including even those with a strong Marxist faith in what he calls "default materialism" and "the bedrock belief in social determination" (191)—who took the linguistic turn that was to have such a powerful impact upon the practice of history down to the present. To be sure, there is a fairly broad sense of dissatisfaction at the moment with some of the limitations of linguistic turn historiography, as evidenced in Victoria Bonnell and Lynn Hunt's edited volume *Beyond the Cultural Turn*; a slew of articles by William Sewell, Richard Biernacki, Miguel Angel Cabrera, Patrick Joyce, William Reddy, Nicholas Dirks, and Marshall Sahlins; the writings of British sociologist Anthony Giddens and German sociologist Andreas Reckwitz; the debate on "The New Empiricism" recently published in volume 1 of *Cultural and Social History*; and other works too numerous to cite.[3] Clearly, the time is ripe for a reconsideration of the relationship of social history to cultural history, and for a rethinking of the fundamental, if not necessarily exclusive, differences in historiographical epistemologies and practices to which each tends.[4] In large part, this is what Eley's *A Crooked Line* sets out to offer.

At stake here is whether and how the basic grounding of social history in historical materialism can be recuperated after twenty-five years of allegiance to a belief in the

comprehensive approach to history in its entirety that would honor the analyses generated by social and cultural logics equally. The "crooked line" constitutes a figure derived from Bertolt Brecht's maxim that "when there are obstacles, the shortest distance between two points—here from cultural history to the history of society—is a crooked line."

[3] Victoria E. Bonnell and Lynn Hunt, eds., *Beyond the Cultural Turn: New Directions in the Study of Society and Culture* (Berkeley, Calif., 1999). William H. Sewell, Jr., "A Theory of Structure: Duality, Agency and Transformation," *American Journal of Sociology* 98 (1992): 1–29; Sewell, "The Concept(s) of Culture," in Bonnell and Hunt, *Beyond the Cultural Turn*, 35–61; Sewell, "Historical Events as Transformations of Structures: Inventing Revolution at the Bastille," *Theory and Society* 25 (1996): 841–881. These, together with newly written and collected articles, have recently been published by Sewell in *Logics of History: Social Theory and Social Transformation* (Chicago, 2005). Richard Biernacki, "Language and the Shift from Signs to Practices in Cultural Inquiry," *History and Theory* 39 (2000): 289–310; Biernacki, "Method and Metaphor after the New Cultural History," in Bonnell and Hunt, eds., *Beyond the Cultural Turn*, 62–92; Biernacki with Jennifer Jordan, "The Place of Space in the Study of the Social," in Patrick Joyce, ed., *The Social in Question: New Bearings in History and the Social Sciences* (London, 2002), 133–150. Miguel A. Cabrera, "On Language, Culture and Social Action," *History and Theory* 40 (2001): 82–100; Cabrera, "Linguistic Approach or Return to Subjectivism: In Search of an Alternative to Social History," *Social History* 24 (1991): 74–89; Cabrera, *Postsocial History: An Introduction*, trans. Marie McMahon, foreword by Patrick Joyce (Boulder, Colo., 2004). Patrick Joyce, "The Imaginary Discontents of Social History: A Note of Response to Mayfield and Thorne, Lawrence and Taylor," *Social History* 18, no. 1 (1993): 81–85; Joyce, "The End of Social History?" in Keith Jenkins, ed., *The Postmodern History Reader* (London, 1997), 341–365; Joyce, "History and Postmodernism," *Past and Present* 133 (1991): 204–209; Joyce, "More Secondary Modern Than Postmodern," *Rethinking History* 5 (2001): 367–382; Joyce, "What Is the Social and Why Is It in Question?," the introduction to his recently edited collection of articles *The Social in Question: New Bearings in History and the Social Sciences* (London, 2002). William M. Reddy, "The Logic of Action: Indeterminacy, Emotion and Historical Narrative," *History and Theory* 40 (2001): 10–33. Nicholas B. Dirks, "Is Vice Versa? Historical Anthropologies and Anthropological Histories," in Terrence J. McDonald, ed., *The Historic Turn in the Human Sciences* (Ann Arbor, Mich., 1996), 17–51. For Sahlins, see especially the articles collected in Marshall Sahlins, *Culture in Practice: Selected Essays* (New York, 2000), as well as Sahlins, *Islands of History* (Chicago, 1985). Anthony Giddens, *The Constitution of Society: Outline of a Theory of Structuration* (Berkeley, Calif., 1986). Andreas Reckwitz, "Toward a Theory of Social Practices: A Development in Culturalist Theorizing," *European Journal of Social Theory* 5 (2002): 243–263; Reckwitz, *Die Transformation der Kulturtheorien: Zur Entwicklung eines Theorieprogramms* (Weilerswist, 2000). *Cultural and Social History* 1, no. 2 (May 2004), in particular the essay by Carla Hesse on 201–207.

[4] For a set of readings and an introductory overview of this development, see Gabrielle M. Spiegel, ed., *Practicing History: New Directions in Historical Writing after the Linguistic Turn* (New York, 2005).

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linguistic and cultural construction of reality. Like so many others at the moment, Eley wishes to rethink the historian's understanding of his or her practice in a way that acknowledges the powerful insights that a linguistic approach to society and culture has offered, yet to revise it from the perspective of a greater focus on questions of how society undergoes constant transformations in both its material and conceptual realms. This concern to reintroduce a social and materialist perspective in historical analysis foregrounds issues of individual agents, historical actions, and the structural constraints that both enable and delimit experience, issues around which much of the debate currently revolves. At base, the question turns on what we believe history is and how it happens. Any answer to this question, and hence to the possibility of recuperating a social and materialist perspective without abandoning cultural history, will be shaped in part by how one thinks the turn to culture came about, what motivated it (besides the accumulating difficulties in social history that Eley narrates so well), and on what grounds social history can be reinstated.

In Eley's view, a decisive shift from the centrality of social history to that of cultural history occurred around 1980, as a new generation of historians trained in the 1960s and early 1970s came to professional maturity. The "disappointment" that characterizes Eley's account of this shift in chapter 2 is attributed to their relinquishing the conviction that "class relations are *the* constitutive element in the history of industrialized capitalist states, the Marxist social historian's axiomatic wish" (110–111). Under the impact of changing political contexts—and especially the rise of feminism and women's/gender history, with its profound concern for questions of subjectivity that had been banished from the then-governing paradigms of social history—there occurred a turn to linguistically conceived forms of cultural history, which split the generation between those who remained committed to what Eley characterizes as "a restlessly aggrandizing social history" and those who came to define themselves as cultural historians, that is, who focused on discourse and its operation in the cultural construction of social life.

Eley believes that "if we write the intellectual history of the discipline honestly . . . we'll find the new impulses coming from the outside" (191). This is largely accurate, I think, although it tends to grant an exceptional status to what is probably more realistically seen as the profession's normal interdisciplinary promiscuity, and to discount the extent to which historians of all stripes, instead of relying solely on rather impoverished traditional theories for "doing history," routinely tend to read in and draw upon cognate fields.[5] As Eley himself notes, "the boundaries separating history from other academic disciplines and from wider influences in the public sphere have been far more porous than the curmudgeonly defenders of history's integrity can ever allow themselves to see" (191–192).

Together with the influence of feminist writings on gender, Eley stresses the importance of the work of Michel Foucault to the emergence of cultural history. Not only did Foucault's early work demonstrate the operation of discourse, or what Foucault called "epistemic regimes," in defining the conditions of possibility for what can and cannot be thought in particular historical epochs (defined by the episteme of an

[5] For a fuller discussion of this point, see Sewell, *Logics of History*, especially chap. 1, "Theory, History and Social Science," and chap. 2, "The Political Unconscious of Social and Cultural History, or, Confessions of a Former Quantitative Historian."

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era and the ways in which it produced the "already encoded eye"),[6] but his elaboration of the idea of the indissoluble connection between knowledge and power (or what is sometimes called the knowledge/power nexus) also formulated a new understanding of power as de-centered and dispersed as a "microphysics" throughout the entire range of society and its social practices, hence challenging the utility of social history's conventional focus on class and the state as the centers of domination and power. As Eley explains, the thrust of Foucault's work was to undermine the materialist view of society and culture in favor of linguistic analysis, a movement aided, to be sure, by the rise of narrativist schools of history indebted in various ways to Hayden White's *Metahistory*, as well as to Jacques Derrida and deconstruction, although in a less thorough way. Added to this mix was the symbolic anthropology popularized among historians by the writings of Clifford Geertz. Although Geertz's brand of symbolic anthropology insisted on its grounding in the social materials of a given culture, when deployed by historians it was the formal patterns, the modes of representation, rather than the social conflicts whose symbolic expression and resolution they served, that tended to become the object of investigation. The result, inevitably, was an aestheticizing of culture and its absorption into the ever-widening category of "textuality" and discourse as poststructuralism came to view it. Additional stimuli to "take the turn" came, Eley believes, from cultural studies, from the somewhat brief episode of the history of *mentalités* promoted by the *Annales* school, by the rise of symbolic anthropology, and by the new prominence given to questions of race and empire and related questions of colonialism and postcolonialism, although in the latter case the impact was less direct until a slightly later period.

It is worth remarking that at the time these developments were making their way into the historian's field of view, clear, critical distinctions between them tended to get lost in the rush to embrace the new epistemologies and methodologies they encompassed. Thus, for example, terms such as "postmodernism" and "poststructuralism" tended to be used interchangeably, and to be conflated with the symbolic anthropology being developed by Geertz. While all the "schools" shared a fundamental reliance on semiotics as the covering explanation for how language operated to mediate the relationship between text and reality, the difference between cultural analysis and the linguistic turn tended to be occluded. Whereas linguistic turn historiography proclaimed culture as a self-enclosed, non-referential mechanism of social construction that *preceded* the world and rendered it intelligible by constructing it according to its own rules of signification, cultural history never abandoned a belief in the objective reality of the social world, and thus might more profitably have been labeled sociocultural history. Eley tends to perpetuate this confusion by equating cultural history with the linguistic turn, although by now the distinctive nature and traditions that went into their making are far better understood.[7]

As a result of the combined influence of these developments, Eley indicates, cultural history achieved its day in the sun. But the light of that sun is now fading, and, he argues, "we don't have to reinstate the primacy of social explanation and a

[6] The phrase comes from Michel Foucault, *The Order of Things: An Archaeology of the Human Sciences* (New York, 1973), xxi.

[7] See, for example, the conflation of the two terms on pp. 125 and 156; in the latter case they are presented as synonymous.

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materialist model of social determination, or insist on the causal sovereignty of the economy and material life, in order to take seriously the tasks of social significance or social analysis." It is time, he believes, to reassert the importance of social history, in order "to keep relating our particular subjects to the bigger picture of society as a whole—whether we are social historians, political historians, cultural historians, or whatever." We can "hold on to all of the gains of the new cultural history without having to abandon everything we learned as social historians" (11). We have, thus, no need to choose between cultural history and the history of society—the terms posed in the book's subtitle—but can, instead, draw equally (and with equanimity) on both the older school of social theorists and the new cultural historian's deployment of discourse as the determinative force in social construction.

I have two principal queries about the thrust of this narrative, although in fundamental ways I share its basic desire for a historiography that acknowledges both the social, contextual determinants of thought and behavior in the past and the mediating role played by language and culture in their functioning—that is, for what David Nirenberg has suggested that one might call a "unified field theory" of history.[8] The first question has to do with Eley's account of the rise of linguistic turn/cultural history, and the second is: What next? What happens if we simply stipulate to the basic argument that there is no need to choose? What kind of history based on what epistemologies and methodologies do we then practice? These questions are related in unexpected ways, at least in their implications for where historiography is likely to go from here.

Although Eley pays homage throughout the book to the decisive influence of French writers and philosophers in effecting the global changes in historical thought and writing in the period between the 1970s and 1990s, noting the work of Louis Althusser, Julia Kristeva, Jean-François Lyotard, and Jacques Derrida, as well as Foucault (e.g., 160), there is little sense in his narrative of the degree to which first French structuralism, then French poststructuralism, was intellectually motivated by a rejection of phenomenology. One need only recall Foucault's own early encounter with Saussure in the late 1940s: he was attending lectures by Maurice Merleau-Ponty when, discussing the problem of subjectivity, he said: "I remember clearly [that] . . . the problem of language appeared and it was clear that phenomenology was no match for structural analysis in accounting for the effects of meaning that could be produced by a structure of the linguistic type, in which the subject (in the phenomenological sense) did not intervene to convey meaning."[9] The subject, instead, like the meaning conveyed, was an "effect" of discourse, a position assigned by and within discursive practices. Thus what was paramount and primary was discourse, as Foucault made abundantly clear in the early chapters of *The Archaeology of Knowledge*, announcing his decision to "abandon any attempt to see discourse as a phenomenon of expression." "Discourse," he said, "is not the majestically unfolding manifestation

[8] David Nirenberg, personal communication. For a theoretical analysis of the grounds on which one might begin to articulate such a field, see Gabrielle M. Spiegel, "Towards a Theory of the Middle Ground: Historical Writing in the Age of Postmodernism," in Carlos Barros, ed., *Historia a debate*, 5 vols. (Santiago de Compostela, Spain, 1995), 1: 169–176, as well as Spiegel, "History and Post-Modernism: IV," *Past and Present* 135 (1992): 194–208.

[9] In Michel Foucault, *Politics, Philosophy, and Culture: Interviews and Other Writings, 1977–1984*, ed. Lawrence D. Kritzman, trans. Alan Sheridan et al. (New York, 1988), 21.

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of a thinking, knowing subject, but, on the contrary, a totality in which the dispersion of the subject and his discontinuity with himself may be determined."[10] Hence the famous "death of the subject," and with it of the materially grounded historical agent.

All else flowed naturally from this key concept of discourse and the novel understanding of subjectivity, with its ability to throw the concepts of agency, experience, and practice into disarray, since absent a purposive historical actor and any concept of intentionality, it became impossible to establish a ground from which the individual could fashion his or her destiny on the basis of his or her experience of the world. The philosophical basis of structuralism and poststructuralism was laid down in the French rejection of phenomenology—of an actor-centered understanding of how the world is perceived and understood—and the adoption of semiotics as the governing paradigm for understanding language, culture, and society, a paradigm later modified, to be sure, by Derrida, deconstruction, and other varieties of poststructuralism, but no less linguistic, for that, in its orientation. Yet semiotics is not, it should be noted, a category that occupies much, if any, space in the book, apart from a necessary role in explaining the rise of the linguistic turn; its adversarial relation to phenomenology is, to the best of my recollection after two readings, altogether absent from the discussion.

In my understanding of the current situation in history and theory, a large part of the revisionist critique of linguistic turn historiography and cultural history and the attempt to move "beyond the cultural turn" is taking its stance on a neo-phenomenological approach that seeks, as Pierre Bourdieu explains (although he dissents from its analytic utility), "to make explicit the primary experience of the social world, i.e. all that is inscribed in the relationship of familiarity with the familiar environment, the unquestioning apprehension of the social world, which, by definition, does not reflect on itself and excludes the question of the conditions of its own possibility."[11] Insofar as they share this view, many historians are deploying a (largely implicit) concept of "social phenomenology" in which, as the German sociologist Andreas Reckwitz explains,

> The aim of social analysis is to take over the "subjective perspective," i.e. to reconstruct the sequence of mental acts of consciousness, which are located "inside" and are directed in the form of phenomenological "intentionality" at outward objects to which the consciousness ascribes meanings. The social then is . . . the subjective *idea* of a common world of meaning . . . The aim of social-as-cultural analysis from the point of view of social phenomenology is to describe the subjective acts of (mental) interpretations of agents and their schemes of interpretation.[12]

Among historians, the reinsertion of the agent as an effective social actor has been achieved by highlighting the disjunction between culturally given meanings and the individual uses of them in contingent, historically conditioned ways. Work done in this vein tends to focus on the adaptive, strategic, and tactical uses made of existing cultural schemes by agents who, in the very act of deploying the elements of culture, both reproduce and transform them. Historical agency, from this perspective, rep-

[10] Michel Foucault, *The Archaeology of Knowledge and the Discourse on Language*, trans. A. M. Sheridan Smith (New York, 1972), 55.

[11] Pierre Bourdieu, *Outline of a Theory of Practice*, trans. Richard Nice (Cambridge, 1977), 3.

[12] Reckwitz, "Toward a Theory of Social Practices," 247.

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resents the individual's relationship to the cultural order, "the embodiment of collective powers in individual persons," as Marshall Sahlins puts it.[13] It is this actor-centered perspective, a belief in individual perception as the agent's own source of knowledge about, and action in, the world—a perception mediated and perhaps constrained but *not* wholly controlled by the cultural scaffolding or conceptual schemes within which it takes place—that I see as the return of a modified phenomenology. If most historians—especially those who still believe in the power and utility of the linguistic turn—have no desire to return to an "objective" social science model of history, that is, to "save the phenomena," many are nonetheless engaging in a widespread attempt to save the phenomenological.

Attendant on this shift is a new emphasis on semantic, as opposed to semiotic, constructivism, in which meaning occurs not on the level of code or structure, but on that of the semantics of ordinary language use, constructing the world through its continual and practical creation and re-creation over time, since no past use of a term determines its application to the next case. By focusing on the adaptive, practical enactment of cultural constituents by individual agents, the varying modalities of use account for how culture is sustained, mediated, replicated, and changed. Hence neo-phenomenology gives rise to a theory of "practice," which emphasizes both the mental and bodily acts undertaken by historical actors, in which, as Richard Biernacki argues, "agents call on bodily competencies that have their own structure and coordinating influence, incorporating corporeal principles of practical knowledge."[14] In this way, everyday practices combine to construct the "socially informed body," which, in its incorporated state, possesses "the instruments of an ordering of the world, a system of classifying schemes which organizes all practice and of which the linguistic scheme . . . is only one aspect."[15]

In this view, culture emerges less as a systematic structure than as a repertoire of competencies, a "tool kit," a regime of practical rationality, or a set of strategies guiding action, whereby symbols/signs are mobilized to identify those aspects of an agent's experience which, in this process, are made meaningful, that is, experientially "real." Culture is thereby recast as a "performative turn," one realized only processually as "signs put to work" to "reference" and interpret the world. Historical investigation, from this perspective, would take practices (not structure) as the starting point of social analysis, and practice itself assumes the form of a sociology of meaning, or *sémantique des situations*, as Bernard Lepetit calls it.[16]

These attempts to modify the totalizing grasp of discursive regimes on social behavior from the point of view of agency, experience, and practice seem indicative of the theoretical negotiations inherent in what I am tempted to call an "accommodationist" strategy governing much of the critique of the linguistic turn. Eley's plea for theoretical/methodological pluralism, including a revitalized social history that seeks to encompass the history of society as a whole, also takes its place here, but on somewhat shaky theoretical grounds.

Toward the end of the book, Eley reiterates his belief that there is "no need to

[13] Marshall Sahlins, "Introduction," in Sahlins, *Culture in Practice*, 25.
[14] Biernacki, "Method and Metaphor after the New Cultural History," 75.
[15] Bourdieu, *Outline of a Theory of Practice*, 124.
[16] Bernard Lepetit, "Histoire des pratiques, pratique de l'histoire," in Lepetit, ed., *Les Formes de l'expérience* (Paris, 1995), 14.

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choose" between social and cultural approaches to history, and concludes, in a heartfelt *cri de coeur*, with a plea for us to recognize "the urgent need for a basic [methodological, epistemological?] pluralism" (201). Indeed, he notes, he has "deliberately avoided any detailed explicating of the various debates surrounding the big shift from social to cultural history that forms the subject of this book," presumably because he finds theoretical debates and "desires for theoretical purity" (his term) unproductive, at least insofar as they tended to polarize the profession during the 1980s and 1990s, at the height of the debates over linguistic turn historiography. Nor is Eley especially sympathetic to an attempt to seek a middle ground, a phrase that he asserts "usually implies a fudged and compromising moderation or type of confusion, a disablement before difficulty or reluctance in taking a stand" (100). To the extent that his pluralistic posture represents a rejection of the theoretical debates of the last few decades, it fulfills, I infer, the promise of "defiance" under whose rubric it is articulated.

But methodological and epistemological pluralism is not—nor, in all likelihood, is it intended to be—a genuinely theoretical position. One might legitimately query, then, whether it can furnish the basis on which to rethink the complex relations between social and cultural modalities of analysis that *A Crooked Line* so fervently advocates. Eley's plea on behalf of theoretical pluralism and the absence of a need to choose circumvents without comment the problem of the different epistemologies at play in an empirically grounded social history and/or a linguistically mediated cultural history. The former implicitly reverts to that "noble dream" of an objective basis for historical investigation, one that, as Peter Novick so ably demonstrated, is no longer shared by most historians, however much we respect and insist on the empirical basis for all historical investigation.[17] The latter entails at least a partial reliance on a semiotic understanding of the constructed nature of our apprehension of that very social reality. This is not to argue that history as a discipline is necessarily confined to the deployment of a single epistemological framework as it shifts its foci of attention and objects of research. At a minimum, the play of scale as one moves from micro- to macro- (or global) analyses of historical phenomena often involves changing epistemological frameworks, however unremarked in the literature.[18]

The most interesting attempts to achieve the kind of dialectically balanced analytic that Eley advocates, one that embraces the insights of poststructuralism while modifying them so as to include a sense of the significance of the social and its instrumental force in human history and thought, are coming at the moment from historians such as William Sewell, who are struggling to devise a social and historiographical theory capable of addressing precisely the complex of ideas that Eley engages. Sewell argues on behalf of a dialectical understanding of culture as the interplay between system and practice in social life, the former understood structurally but modified in its effects by the contradictory, contested, and constantly changing ways in which it is implemented in the latter.[19]

Hence Sewell approaches questions of the role of events and individual and col-

[17] Peter Novick, *That Noble Dream: The "Objectivity Question" and the American Historical Profession* (Cambridge, 1988).

[18] One of the few works to systematically investigate this problem that I am aware of is Jacques Revel, comp., *Jeux d'échelles: La micro-analyse à l'expérience* (Paris, 1996).

[19] See Sewell, "The Concept(s) of Culture," 53ff. See also his new book *Logics of History*.

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lective behavior in phenomena such as the taking of the Bastille—and the revolutionary consequences that followed upon it—by understanding them as participating in a dialectic of system and practice through which the existing cultural order is rearticulated and transformed. To engage in any form of social, political, or cultural behavior means drawing upon a set of socially conventional, thus commonly shared, meanings in order to be understood and to be consequential. In that sense, symbolic interpretation, Sewell insists, "is part and parcel of the historical event," since actions have meaning only in relation to the cultural order within which they occur.[20] At the same time, the system as such exists solely in the continuity bestowed upon it by the succession of practices that bring it to life. But every practice inflects and changes the system upon which it draws and which it instantiates. In an event as momentous as the taking of the Bastille, the result is a transformative rearticulation of the underlying conceptual structures that guided French society in the *ancien régime*, creating novel systems of signification among which "the Bastille," the "revolution," "despotism," and the like take their place.[21] Critical to this process is the notion that inherited languages (or discourses) can never fully encompass or adequately describe the vast variety of empirical realities or experiences presented to the social actor for categorization and interpretation, and that, in that sense, life outruns the capacity of culture to account for it.[22]

No less eclectic than Eley in the sources upon which it draws, there is, nonetheless, among the historians attempting to rethink the question of structure and practice, an effort to engage the underlying theoretical issues, if only to join in Eley's sense of the refreshing benefits to historical knowledge that ensue from such endeavors. As Eley himself points out, there is a body of recent work by younger scholars that "specifically refuse[s] the polarized division between the 'social' and the 'cultural,' vesting 'recognizably social and political topics with a cultural analytic, responding to the incitements of cultural theory, and grounding these in as dense and imaginative a range of sources and interpretive contexts as possible'" (201). It is doubtful that this work will ever again seek the totalizing goal, also espoused by Eley, of "grasping society as a whole"—of moving in "a crooked line" from cultural history to the "history of society"—given the wholesale dissolution of what he earlier called "social history's totalizing aspiration" (193). However, an exploration of the theoretical bases, both epistemological and methodological, on which to generate the logics of history (to borrow Sewell's terminology) and historiography intrinsic in this movement doubtless will prove important to its ultimate shape.

In conclusion, to join my first and second questions, I would argue that just as the linguistic turn, hence cultural history, grew out of a critique of phenomenology, so one strand of the current revisionist thrust to much historical theorizing is collecting itself under the banner of a neo-phenomenological approach, latterly grouped, at least by Reckwitz, under the rubric of "Practice Theory." Drawing on diverse, and often incompatible, congeries of theories—which include Pierre Bourdieu's project of a "praxeology" and its semiotic variant found in Michel de Certeau; Anthony Giddens's "theory of structuration"; the "ordinary language" investigations

[20] See Sewell, "Historical Events as Transformations of Structures," 861.
[21] Ibid.
[22] For a discussion of this, see Sahlins, *Islands of History*, 147–148.

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of late Ludwig Wittgenstein; and a deeper understanding of Foucault's late work on governmentality and bio-politics, which intersects with theories of the body, both feminist and sociological—combined with the neo-hermeneutical model of embodied agency that derives much of its force from the ethnomethodological models supplied by ethnographers such as Harold Garfinkel, "Practice Theory" asserts the continuing relevance of semiotic insights proffered by the linguistic turn, yet reinterprets them in favor of a rehabilitation of social history by placing structure and practice, language and body in dialectical relation in historical systems.[23] In that sense, as Victoria Bonnell and Lynn Hunt have argued, it would appear that scholars are engaged in a redefinition and revitalization of the concept of the "social" that had been weakened by poststructuralism.[24]

It is in this sense that the "linguistic turn" is giving way to a "historical turn," since historicism—understood as the acknowledgment of the contingent, temporally and socially situated character of our beliefs, values, institutions, and practices—subtends both the retention of an attenuated concept of discourse as that which creates the conditions of possibility for, and the constituents of, a given culture, and the revisionist emphasis on practice, agency, experience, and adaptive uses of historically specific cultural resources. It achieves this primarily by (re-) adopting an actor-centered perspective that necessitates an understanding of the *social* as well as symbolic ground from which texts and behaviors of all kinds are generated, and through its attention to the ways in which practice continually modifies the system within which it operates. Whether such an approach deserves the name "neo-phenomenological" is for the reader to determine, but it seems clear that any "return to reality" will include a consideration of the ways in which individual and collective social actors operate, based upon their perceptions and understandings of both the social and symbolic systems that govern behaviors and endow them with socially significant meanings.

Speaking as someone who remains profoundly committed to the insights and analytic rewards for historical analysis proffered by the linguistic turn in historiography, it remains for me an open question whether such revisionist moves are likely to succeed, that is, to offer a persuasive modification of the linguistic turn, and whether this recent turn in historiography represents a final—exhausted—phase in the reception of linguistic turn historiography or a genuinely novel initiative that strikes out in new directions. As I argued back in 1990:

> The ability of semiotics to sweep the theoretical field was testimony to the power of its challenge to traditional epistemologies, to the technical virtuosity of its practitioners, and to the underlying coherence of its theory, against which those advocating a return to history rather weakly invoke collective "common sense" or individual subjective experience. But while there are good historical reasons for historians to insist on the autonomy of material reality, they are not necessarily reasons which make for good history, and the linguistic turn cannot be met simply by an appeal to common or individual sense and experience—nor, I would add now, to social history tout court.[25]

[23] See especially Michel de Certeau, *The Practice of Everyday Life*, trans. Steven Rendall (Berkeley, Calif., 1988); Giddens, *The Constitution of Society*; Harold Garfinkel, *Studies in Ethnomethodology* (Cambridge, 1984).

[24] Bonnell and Hunt, *Beyond the Cultural Turn*, 11.

[25] Gabrielle M. Spiegel, "History, Historicism and the Social Logic of the Text," *Speculum* 65 (1990):

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Eley's book takes a stand among current demands to recuperate the material, in effect, the social, as part and parcel of a belief in the reality and socially significant presence of the past, both in the past and in the present. If I quibble about his account of the etiology of these debates and developments, it only furnishes a further illustration of his principle that our approaches to historiography are inevitably personal, governed by the particular contexts of our own histories, politics, and professional commitments.

59–86. Reprinted in Spiegel, *The Past as Text: The Theory and Practice of Medieval Historiography* (Baltimore, 1997), 19.

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AHR Forum
Remembering the Future

MANU GOSWAMI

GEOFF ELEY IS A PROMINENT NEW-LEFT SCHOLAR of modern German and European social history. *A Crooked Line* is his meditation on the relationship between his intellectual biography, political transformations, and the historiographical shift from the social history of the 1960s and 1970s to the cultural turn of the 1980s and 1990s. The book is self-consciously hybrid. Its bold braiding of personal memoir, historiographical analysis, and political critique places it on the terrain of the cultural turn, making it an artifact of the shift it charts from social to cultural history. Yet its emphasis on the political context of social and cultural history remains true to the basic tenets of social history. The hybrid character of the book encapsulates its concluding, and controversial, claim that contemporary historical practices have so combined social and cultural history that they obviate any "need to choose" between them (181).

Eley's work joins a personal and intellectual memoir with an old-school Hobsbawmian mapping of historical debates across British, German, European, and South Asian fields. The heterodox nature of the work is signaled by the affective register of the titles of its chapters—"Optimism," "Disappointment," "Reflectiveness," and "Defiance." The temporal structure of the book's narrative is similarly heterodox. The work moves between the political and existential dimension of becoming a Marxist social historian in the 1960s and 1970s, to a more sweeping historical panorama of the interchange between politics and historiographical debates in postwar Britain, Germany, and the United States, to a concluding affirmative snapshot of contemporary historical practices. This remarkable compass affords a rare demonstration of the uneven and multiple times—political, generational, and existential—that underwrite *critical* historiographical stocktaking. The experimental form of the book suggests that the only way to render intelligible the shift from social to cultural history is to short-circuit linear narratives in favor of Bertolt Brecht's concluding injunction, voiced by Galileo, that "If there are obstacles, the shortest line between two points may be the crooked line."

It is tempting to read *A Crooked Line* as the reckoning of one historian—or the post-1968 generation of historians—with the shift from social to cultural history. But while that shift was made by a specific generation, its sources and consequences extend far beyond that generation. To me, a historian of modern India trained in the mid- and late 1990s, when cultural studies and postcolonial history were at their peak, the force of the book lay in its explicitly political rewriting of that momentous shift. Eley offers a generous and generative reading of social and cultural history as

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contextually specific instances of "insurgent politics." Yet against his celebration of contemporary cultural history, I suggest that attention to the broader temporal politics of historiography reveals a central loss sustained in the cultural turn. I argue that the project of social history was underwritten by a transformative, future-oriented vision and that the absence of this orientation limits the capacity of cultural history to critically engage the historical present. In extending this argument to Eley's discussion of subaltern studies, I identify the eclipse of political economy and of comparative frameworks as linked casualties of the cultural turn, and suggest how this hinders understanding of a historical present marked by the renewal of empire and the expansion of neoliberalism.

ELEY OPENS WITH A VIVID ACCOUNT OF HIS EMBRACE of the class-centered British Marxian model of social history. This project cut against the grain of the insular structure of university disciplines in Britain in the late 1950s and 1960s, the class inequalities of the Fordist-Keynesian model of capitalism (mass assembly line, mass political organization, and a regulatory welfare regime), and the "fact-grubbing theory-averseness" of received historical research (15). Against the reconstructive, backward-looking stance of traditional historiography, materialist historians elaborated a conceptual framework that enshrined a radical notion of politics as constitutive of history. The "breakthrough to social history was unimaginable," Eley observes, "without the sense of political possibility beckoning during the later 1960s" (59). The political impetus of social history, exemplified by E. P. Thompson, directed the appropriation of the suppressed dimensions of past struggles that were seen to contain the possibility of a retrospective redemption in the present. The capacious materialism of social history bore a strong affinity with the structuralism of the French *Annales* school and the consolidating schema of Weberian historical sociology in the United States. During the 1960s and early 1970s, the interlacing of these frameworks underwrote the comparative and macro-scalar focus of an expansive terrain of interdisciplinary research.

Eley traces the decline of this social history project to developments in the late 1970s and the 1980s: the attenuation of labor movements; the disappearance of socialist revolution from the narrative of history; the reconfiguration of state-society relations in the context of deindustrialization, rampant unemployment, and monetary crises; and the apparent explosion of social movements that articulated gender, race, and class struggles in striking new ways. These transformations undermined the empirical plausibility of key tenets of British Marxism, especially its conception of class conflict as the structuring dimension of historical change and its attribution of world-historical agency to the working class. By the early to mid-1980s, the mounting perception of a *de facto* historical impasse had transmuted into theoretical terms: the state-centered paradigm of a unified history, the teleological conception of historical time, the analytical primacy accorded to class relations, and objectivistic conceptions of subjectivity no longer carried a presumptive status. The difficulty of redeeming political failures methodologically or more precisely within the terms of class-centered Marxism provoked an acute collective "disappointment." And it was in this conjuncture that the cultural turn occurred.

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The sources of the cultural turn are many and resist easy summary. Particularly important were feminism and its forging of new categories of analysis and narrative forms (Eley cites Carolyn Steedman and Joan Scott as exemplary figures); the Foucauldian-inspired discursive revolution in the concept of the archive and the related rejection of inherited nineteenth-century models of history in favor of genealogical critique; and the mobilization of Foucault and poststructuralism by postcolonial historians claiming that colonialism was the unacknowledged basis of prevailing conceptions of modernity, subjectivity, and historical time. These intellectual developments were tied, in ways not explored by Eley, to the reception of structuralist semiotics, Saussurean and post-Saussurean, from France in the early 1970s. Indeed, Althusser's theory of ideology and Foucault's conception of discourse had already pointed toward the extension of post-Saussurean semiotic models in historical and sociological research. What held feminist, Foucauldian, and postcolonial approaches together was a shared epistemological foundation in semiotics. Cultural history posited signifying practices as the key to understanding power, textuality as a privileged medium and object of interdisciplinary analysis, and language and representational practices as the only bases for an anti-totalizing history. While Eley elides a discussion of the semiotic inflection of cultural history, his contrapuntal narration of emotional responses, political investments, and fiercely waged scholarly debates provides a visceral sense of the entwinement of politics and history.

BY EMPHASIZING THIS INTIMACY, ELEY RENDERS SOCIAL AND CULTURAL HISTORY as contextually different forms of "insurgent politics." But in order to assess the conceptual stakes entailed in paradigm shifts, we need to pay closer attention to the wider temporal politics of historiography or how social and cultural historians envisage the relationships between past, present, and future in substantively different ways. *A Crooked Line* does not engage these questions in an explicit manner. But this line of inquiry is invoked with particular force in its account of the rise of social history. Eley's impassioned guided tour of British social historians—Christopher Hill, E. P. Thompson, Eric Hobsbawm, and Raymond Williams—does more than underline the fact that Marxism served, in Frederic Jameson's phrase, the role of the "vanishing mediator" in the rise of cultural history and cultural studies in the humanities at large.[1] Revisiting this archive, in Eley's able company, reminds us that social history emerged at a time when collective political contention combined with historical materialism to offer a very different understanding of the future from that on offer today.

The totalizing impulse of social history found varied expression in Eric Hobsbawm's "history of society," Raymond Williams's "whole way of life," and Charles Tilly's "big structures," "large processes," and "huge comparisons."[2] It was tied to a conception of politics as the extended temporal constitution—the willed production, reproduction, *and* transformation—of the ensemble of social relations.

[1] Frederic Jameson, "The Vanishing Mediator: Narrative Structure in Max Weber," *New German Critique* 1 (Winter 1973): 52–89.

[2] Eric Hobsbawm, "From Social History to the History of Society," *Daedalus* 100 (Winter 1971): 20–45; Raymond Williams, "Culture Is Ordinary," in Norman Mackenzie, ed., *Conviction* (London, 1958), 93–120; Charles Tilly, *Big Structures, Large Processes, Huge Comparisons* (New York, 1984).

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Within the terms of historical materialism, a future-oriented horizon gave meaning to the historical present, just as the contradictions of the present both framed efforts to conjure the past and opened up the prospect of a radically other future. This conception of the relationships between the past, present, and future drove the quest of reconstructing the history of a social totality and impelled the retrieval of the struggles and experiences of dominated groups. Both ventures were framed in directly political terms; the totalizing impulse was anti-systemic, and the effort at retrieval was understood as an act of collective critical remembrance that contained the potential of mediating between the past and the present, the actual and the possible. This transformative, future-oriented logic exceeded and leavened the historical stagism and sociological schematism of classes and modes of production that haunted orthodox Marxism.

The popular resonance of social history was rooted in its capacity to link together collective experience, especially the realm of aspiration, and historical knowledge. Its status as a critical paradigm resided in its orientation toward histories of the possible rather than just the actual. And for precisely these reasons, it is difficult to concur with Eley's endorsement of contemporary cultural history, much less the categorical claim that a coherent social history paradigm is "simply not available any more" (189). Eley does not specify the precise basis for this claim. It rests in part in a metonymic operation whereby the historiographical fortunes of British Marxism stand in for the "actually existing" heterogeneity of social history and Marxism as a whole. It also equates the prevalence of cultural history with conceptual adequacy as such, calling into question its status as an insurgent formation. Yet against these stated conclusions, the narrative arch of the book points in a different direction. It underscores the deeply consequential character of the shift from a tradition of social history bursting with anticipatory content to a cultural turn burdened by a melancholic conviction that systemic critique and social transformation are impossible. While social history sought to embed the possibility of transformation within the contradictions of a historical present, the cultural turn articulates the negation of a tension between experience (past) and expectation (future), of the possibility of a recognizably other future.

The organizing premises of cultural history—contingency, episodic ruptures unmoored in immanent social contradictions, undecidability, difference, fragmentation—mark a sharp reversal from the collective horizon of expectation that informed and empowered social history. In historiographical terms, this has been expressed in the proliferation of fragmentary histories over analyses of large-scale transformations, in the prevalence of interpretation over causal explanation, and in the axiomatic status of the local, the particular, and the contingent over deep structures, durable inequalities, and persistent hegemonies. In the social realm, it is manifest in the waning of a particular temporal politics, that of imagining a constitutionally different future.

Far from validating declarations of a perpetual present, these shifts should prompt recognition of a troubling dehistoricization within post-Foucauldian cultural history. The dimension of historical experience most deeply affected by the social and representational forms specific to neoliberal capitalism is that of our relation to the future. These social forms include the accelerated and volatile integration of

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financial, labor, and capital markets and the precedence of short-term financial speculation over long-run development horizons. These shifts have occurred in the context of wave after wave of industrial restructuring (deindustrialization in the North and aggressive industrialization in parts of the South) followed by wage stagnation, the spatial dispersal of production, and the dismantling of inherited Fordist-Keynesian and Bandung models of national development and regulation.[3] The ascendancy of neoliberalism across diverse regional and national contexts has been marked by the intensified commodification of the ways and means of everyday life and by the widening of economic inequalities across class, gender, racial, and regional divides. These globally scaled social processes have deepened the technologically shaped primacy of information over narrative meaning and spawned, in turn, a paradoxical surfeit of historical representations and a scarcity of historical consciousness. While these transformations have not been uniform across regions, they have strained efforts to envisage a future beyond that of the present.

The transformative temporal horizon of social history was embedded within a specific historical moment and geopolitical field. The sociohistorical horizon that impelled classical social history is not readily available in the historical present. Yet any attempt to constitute an oppositional, rehistoricizing framework must reckon with the challenges posed by the hollowing out of a collective horizon of expectation that envisioned the seeds of a new future from within the contradictions of a historical present to a neoliberal era seemingly empty of what Pierre Bourdieu called "the simple possibility that things might proceed otherwise."[4] The point here is not to revive the mechanical determinations and tight teleologies that weakened key strands of materialist social history. Rather, it is to underline the importance of exploring what our neoliberal historical present means from the standpoint of the critical and future-oriented framework of social history.

Among the striking achievements of *A Crooked Line* is its delineation of the locally inflected cultural turns instituted across regional fields. There are, to be sure, important exceptions to the move charted in the work—the continued sway, for instance, of social history paradigms within modern Latin American historiography. The vitality of neo-Gramscian frameworks within modern Latin American history, organically linked to the proliferation and strength of diverse social movements in the region, actually provides further ballast for Eley's claim about the reciprocity between political fields and historiographical forms. Yet it is hard to dispute the ascendancy of the cultural turn within the humanities at large.

The fact that so many historians, across many fields, have made the cultural turn suggests that the turn itself is a response to developments outside these fields. What is more, we cannot account for this turn from within the terms of cultural history.

[3] Key works on contemporary transformations include Giovanni Arrighi, *The Long Twentieth Century: Money, Power, and Origins of Our Times* (London, 1994); Pierre Bourdieu, *The Weight of the World: Social Suffering in Contemporary Societies*, trans. Priscilla Pankhurst (Stanford, Calif., 1999); Robert Brenner, *The Boom and the Bubble* (London, 2003); David Harvey, *The Condition of Postmodernity: An Enquiry into the Origins of Cultural Change* (Cambridge, Mass., 1990); Harvey, *A Brief History of Neoliberalism* (New York, 2005).

[4] Pierre Bourdieu, *The Logic of Practice*, trans. Richard Nice (Oxford, 1990), 99.

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For cultural history cannot account for the conditions of its own global emergence and resonance without recourse to the forms of historical totalization that it rejected in the reaction formation against social history. Only a framework attuned to the dynamic interchange between social transformations and categories of perception could pose, let alone undertake, the twin tasks of accounting for the emergence of social and cultural history as distinct standpoints of critique and of demonstrating their immanence within a specific social, political, and intellectual field. It is precisely this task that William Sewell, a protagonist in Eley's narrative, undertakes in a series of recent essays.[5] One of the characteristics of a genuinely critical framework is the ability to think itself, to locate its own categories and concepts in the social and historical context of its emergence.[6] Heeding Eley's call to fashion critical new "histories of society" would require reintroducing the problematic of the social mediation of categories of perception and everyday practices without reifying the social in the form of a fixed historical end or representational unity. For stripped of such mediations, Eley's concluding plea for a "basic pluralism" in historical practice points in any methodological and epistemological direction (201).

IN THE SPECIFIC INSTANCE OF MODERN SOUTH ASIAN HISTORY, Eley notes that subaltern studies "negotiated within its own trajectory, all the aspects of the transition from social to cultural history experienced by historians of Europe in these same years" (144). Indeed, in significant ways, subaltern studies "both presaged and paralleled the course" of historiographical shifts in the "West" (146). There was, in other words, no temporal lag within the trajectory of subaltern studies in making the cultural turn. I share Eley's sense of the achievements vouchsafed for South Asian historiography and beyond by subalternist historians.

From the moment of its emergence in the early 1980s, the subaltern studies collective has challenged narrowly institutional and formalistic conceptions of political subjectivities and popular struggles. In their effort to recover subaltern subjects as more than just the invisible watermarks that haunt the colonial archive, subalternist historians have forged fresh techniques for rereading the colonial archive against the grain. They have sharpened our sense of the social and categorical violence wrought by colonialism on subaltern social groups, of the differential agency of the colonized, and of the limits of ideal-typical and teleological conceptions of modernity in grasping the dynamics of colonial and postcolonial societies. The emphasis on the specificity of colonial and postcolonial societies represented a crucial corrective over accounts inattentive to or straightforwardly contemptuous of difference. During the 1990s, this initial emphasis assumed a much stronger cast with the proliferation of claims of an incommensurable difference. These claims were registered in the shifting fortunes of the category subaltern, which lost its initial Gramscian meaning and increasingly stood in as a sign of untranslatable alterity as such.

This shift roughly paralleled the resignification of the category "Europe," which, as Eley notes, acquired the status of a "particular abstraction" in subalternist his-

[5] William H. Sewell, Jr., *Logics of History: Social Theory and Social Transformation* (Chicago, 2005), 22–80, 318–372.

[6] For a lucid account of reflexivity as a methodological issue, see Pierre Bourdieu, *In Other Words: Essays towards a Reflexive Sociology*, trans. Mathew Adamson (Oxford, 1990).

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tories (194). For many subalternist historians, "Europe" names the developmental historicism that imbues both liberal modernization frameworks and traditional Marxism.[7] Yet for many, the deficiencies of these specific comparative frameworks have taken the form of rejecting comparison as such in favor of a focus on forms of heterogeneity that exceed the representational capacity of dominant historical thought. There is a historically specific seductive pull to the subalternist quarrel with "Europe"; it stems in part from the promise of containing the transformation of received worlds of value and meaning wrought by colonial rule and imperial economic domination. The impossibly overburdened category of "Europe" serves as the site of a utopian desire to territorialize the transformative dynamics linked to colonial rule and imperial capitalism. And it simultaneously propels the complementary move of positing an indigenous realm as a pure externality, a simple outside, to this domain. As initially advanced, not only did the strong claims of incomparability and singularity risk a general inaudibility in political terms, but they were ultimately unintelligible even to themselves. For the notion of singularity is necessarily relational. It can mark the limits of the intelligibility of specific concepts and tropes, but we cannot cognitively grasp singularity as such. Claims of the existence of a privileged point outside the historically constitutive ensemble of colonial or postcolonial relations—whether in terms of an "inner spiritual sphere," untranslatable alterity, or an "alternative modernity"—unwittingly preserve, even as they seek to overcome, normative conceptions of "Euro-America" as the locus of a unitary modernity.[8]

The changing meanings of the term "subaltern" index a wider move within the interdisciplinary arena of colonial studies, away from a prior focus on uneven development, center-periphery relations, and material exploitation toward an emphasis on colonial dialogics, representational strategies, cultural hybridity, and dispersed disciplinary regimes.[9] This cultural turn has sought to move beyond the economism that marked many prior works on colonialism. But it has also been fueled by the conviction that analyses of large-scale processes of transformation and their material underpinnings only legitimate Euro-American imperial narratives. Yet the normative and analytical power of prior materialist frameworks derived in part from a fidelity to the work and practice of many anticolonial intellectuals and movements that were resolutely translocal and materialist in texture and scope. Recent efforts to displace capitalism as a central object of historical analysis for understanding imperial formations, of relegating it to a " 'hyper-real' Europe,"[10] have paradoxically emerged in a historical moment defined by the growing global immanence of neo-

[7] The *locus classicus* of this argument is Dipesh Chakrabarty's *Provincializing Europe: Postcolonial Thought and Historical Difference* (Princeton, N.J., 2000).

[8] For "inner spiritual sphere," see Partha Chatterjee, *The Nation and Its Fragments: Colonial and Postcolonial Histories* (Princeton, N.J., 1993), 6. This work marked a wider turn within subaltern studies from Gramscian to Foucauldian perspectives. For "alternative modernity," see Dipesh Chakrabarty, *Habitations of Modernity: Essays in the Wake of Subaltern Studies* (Chicago, 2002); Dilip Goankor, ed., *Alternative Modernities* (Durham, N.C., 2001); Gyan Prakash, *Another Reason: Science and the Imagination of Modern India* (Princeton, N.J., 1999); Timothy Mitchell, ed., *Questions of Modernity* (Minneapolis, 2000).

[9] For programmatic reviews of colonial studies and histories of empire from different perspectives, see Nicholas Dirks, ed., *Colonialism and Culture* (Ann Arbor, Mich., 1992); and Frederick Cooper, *Colonialism in Question: Theory, Knowledge, History* (Berkeley, Calif., 2005).

[10] Chakrabarty, *Provincializing Europe*, 45.

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liberalism. The contemporary conjunction of renewed projects of empire and neoliberalism seems poised to render frameworks that bracket political economy and what might be called the comparative imperative ever more scholastic. We need to break away from the commonsense notion that attention to the complex dynamics of accumulation and dispossession necessarily returns us to the thralldom of metanarratives characterized by a disregard of the everyday negotiations, expressive action, and creative agency of subaltern subjects. As a number of critical social theorists and anthropologists have shown, it is possible to write histories of capitalism as an internally contradictory, uneven, yet global *social* formation without reducing it to a mode of production, and without treating the economy as an ontological given, as a self-regulating sphere, or as determinative in some dreaded last instance.[11]

FOLLOWING THE GLOBALLY REFRACTED EVENTS OF 1989, a wave of scholarship translated the multiple defeats of the new-left generation into ostentatious claims of the end of history, the end of ideology, the end of the social.[12] In its advocacy of history as a political vocation, *A Crooked Line* indicates why such historical endisms should strain our credulity. It represents a bracing antidote to narratives that either overtly celebrate or uncritically reflect the absence of a systemic alternative to the predominant social and political forms of the present. The "defiant" hope voiced in the work that new "histories of society" might emerge in the wake of the cultural turn joins a growing chorus of calls to break out of the corral of culturalism (203).[13] These efforts might well provide the stimulus for a sustained mediation of social and cultural forms in historical debates and political imagination alike.

[11] For non-economistic conceptions of capitalism, see Pierre Bourdieu, *The Social Structures of the Economy* (Oxford, 2005); Derek Sayer, *The Violence of Abstraction: Analytical Foundations of Historical Materialism* (London, 1987); Karl Polanyi, *The Great Transformation: The Political and Economic Origins of Our Times* (1944; repr., Boston, 2001); Moishe Postone, *Time, Labor and Social Domination* (Cambridge, 1993). Anthropological studies of contemporary and historical capitalism are too numerous to cite but include Jean Comaroff and John Comaroff, eds., *Millennial Capitalism and the Culture of Neoliberalism* (Durham, N.C., 2001).

[12] Some key works include Jean Baudrillard, *The Illusion of the End*, trans. Chris Turner (Stanford, Calif., 1994); Baudrillard, *In the Shadow of the Silent Majorities . . . or the End of the Social*, trans. Paul Foss, Paul Patton, and John Johnson (New York, 1983); and Francis Fukuyama, *The End of History and the Last Man* (Harmondsworth, 1992). For a critique, see Perry Anderson, "The Ends of History," in Anderson, *A Zone of Engagement* (London, 1992), 279–375; and Lutz Niethammer, *Posthistoire: Has History Come to an End?* trans. Patrick Camiller (London, 1992).

[13] Victoria E. Bonnell and Lynn Hunt, eds., *Beyond the Cultural Turn: New Directions in the Study of Society and Culture* (Berkeley, Calif., 1999); Harry Harootunian, *History's Disquiet: Modernity, Cultural Practice, and the Question of Everyday Life* (New York, 2000); Reinhart Koselleck, "On the Need for Theory in the Discipline of History," in Koselleck, *The Practice of Conceptual History: Timing History, Spacing Concepts*, trans. Todd Samuel Presner (Stanford, Calif., 2002), 1–19; Koselleck, "Social History and Conceptual History," ibid., 20–38; and Sewell, *Logics of History*.

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AHR Forum

The Profane and Imperfect World of Historiography

GEOFF ELEY

I WROTE *A CROOKED LINE* BETWEEN THE FALL OF 2003 AND THE SPRING OF 2004 in a rush of urgency and optimism. The urgency came from the parlous political state of the world. In light of recent events, I found myself thinking a lot about the changing bases of intellectual dissent since the 1960s, contemplating in particular the dismal gap between the possible hopes of an earlier time and the difficulties these days of making any connection between what we do in our scholarly lives and what counts as history in the public sphere. For any historian on the Left whose sense of an intellectual and professional calling included an ethics of political involvement—whether in the most direct ways, more modestly via the politics of knowledge, or simply by being open and honest about the salience of political meanings—these seemed very dispiriting times. But the optimism, on the other hand, came from a sense of buoyancy and opportunity in the discipline. The angry divisiveness of some years before, when history had produced its own versions of the culture wars, seemed to be giving way to a more collegial and constructive set of collective conversations, whether field by field or at the level of the discipline per se, not least because younger generations with less of the old baggage were coming to the fore.

One of my book's purposes was certainly to be an exercise in historiographical stock-taking. I wanted to track the complicated passages between social history and cultural history during the past four decades while assembling the bases for judging what has been gained in the process as well as what might have been lost. But my second motivation, perhaps the more important, was to offer a sustained series of reflections on the complexities of history's relationship to politics. That can be a treacherous ground. The last thing I wanted to propose was any straightforward or instrumental type of transmission between the two, and I took great pains in trying to explicate the possible forms the relationship might take. Rather than any one-to-one correspondence, I wanted to stress the gaps and disjunctions, the tensions in history's connectedness to the public worlds of politics, and the necessary spaces of difficulty. I wanted to give due emphasis to the subtle and complicated ways through which political meanings enter the practice of historians, sometimes willingly invited, but just as often guardedly refused. Political meanings frequently arrive as unwelcome intruders. If politics can be inscribed for much of the time indirectly and imperceptibly, at other times the presence can be powerful and very direct, with consequences that can trouble the historian's practice as much as inspire it. There is, in addition, an interior politics of knowledge in the profession and the discipline,

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which involves everything from the structured institutional settings where history is practiced (departments, institutes, universities, classrooms, conferences, journals, networks, associations) to the rules and protocols that define its boundaries and the widest forms of intellectual disputation over its methods, its archives, its schools and traditions, its forms of theory, and its epistemologies. Finally, I wanted to say something about how ideas and assumptions about the past tend to circulate more widely in popular terms across the public culture. All of these are aspects of history's politics.

It was for these purposes that I decided to make modest use of my own story. I thought that this might be helpful in two ways. First, I have been struck over the years by the messy unevenness through which extremely important political changes have registered their impact in my own thinking; and second, I am equally impressed by the time-lagged and bumpy relationship of such political departures to the ways in which I have approached my historical work. This creates a kind of complicated non-synchronicity that can be very disconcerting: no one likes to be made aware of inconsistencies, least of all as they bear on one's fundamental principles or most valued ethical commitments. More commonly we like to see our own thought as a story of the attainment of maturity and the arrival at understanding—as a seamless progression, as growth and enlargement, as a consciously managed working through of a problem to its necessary solution. Yet we seldom experience significant change in so straightforwardly self-conscious and directed a way. In practice, problems creep up on us. They register their presence slowly and partially, taking up residence in hidden or shrouded corners, doing their work behind our backs. Often they ambush our understanding and take us by surprise. In other cases we notice them unexpectedly by means of chance encounters (a seminar we attend, a controversy we observe, a book we happen to read). Usually a wide range of these experiences accumulates before our receptiveness accrues. Where ideas pose threats or challenges to what we think we already understand, resistance and evasions will probably result. Before change can be consummated, a great deal of wrestling will have to occur. After the change, much elaborate redescription will be applied. Consistency and coherence will be rediscovered and projected backward in time. Belatedly, we come to realize what we had really wanted to think.

In pondering how my own thinking has changed over the past few decades, I wanted to find ways of writing an intellectual history of the present that took this quality of unexpectedness and contingency into account. I also wanted to pay honest respect to the difficulties, confusions, and resistances, particularly those that may be unconscious or only partially understood, through which fundamental changes of thought can usually happen. In many respects, moreover, we already possess a detailed record of all the ramifications of the so-called cultural turn.[1] That account

[1] See especially Elizabeth A. Clark, *History, Theory, Text: Historians and the Linguistic Turn* (Cambridge, Mass., 2004); Miguel A. Cabrera, *Postsocial History: An Introduction* (Lanham, Md., 2004); Gabrielle M. Spiegel, "Introduction," in Spiegel, ed., *Practicing History: New Directions in Historical Writing after the Linguistic Turn* (New York, 2005), 1–31; Ann Courthoys and John Docker, *Is History Fiction?* (Ann Arbor, Mich., 2005), especially 137–237. For my own contributions in this mode, see "Is All the World a Text? From Social History to the History of Society Two Decades Later," in Terrence J. McDonald, ed., *The Historic Turn in the Human Sciences* (Ann Arbor, Mich., 1996), 193–243; "Between Social History and Cultural Studies: Interdisciplinarity and the Practice of the Historian at the End of the Twentieth Century," in Joep Leerssen and Ann Rigney, eds., *Historians and Social Values* (Am-

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quite properly centers around the principal theoretical and epistemological issues involved, commonly emphasizing a series of major figures and their contributions, the resulting debates and controversies, and the varying forms of their resolution. The tumults in the discipline and the cognate developments across the wider transdisciplinary map have now been captured in a profusion of guides, commentaries, anthologies, forums, new journals, and a great deal of continuing discussion. Instead of retelling that story one more time, I thought it would be more illuminating to find other ways of writing the intellectual histories involved.

To that end, in the long chapter 4 on the 1980s ("Reflectiveness"), for example, I looked for a series of emblematic books whose impact extended across fields and disciplines, around which widespread and variegated interest converged, whose impact had a symptomatic relationship to the emergent ideas, and which did something to help assemble the cumulative readiness for change during the middle years of the decade. Often preceding the most commonly cited texts, such works possessed their own prehistories of gestation, too—in seminars, conferences, pedagogical settings, and varieties of collaboration cutting across field and disciplinary boundaries. Similarly, I was looking for ways to illustrate the oblique impact of new intellectual departures that may have occurred some distance away from one's immediate scholarly work—in other fields, other periods, other parts of the world, other disciplines—whose direct pertinence for one's own purposes was not so easily apparent, but whose unsettling or enabling potential found its way into one's thinking nonetheless. Feminist critiques of social history and the slow-burning consequences of the rise of women's history provided one primary instance of that effect; a second was the advent of race theory and postcolonial studies at the turn of the 1990s, for which I used whiteness studies and the subaltern studies school as my twin markers.

I wanted to use my own experience of these changes as a way of getting closer to the manner in which they actually took place—by presenting not only the clarity attained in the course of the new departures, but also something of the confusions, false starts, dead ends, and wrong turnings that were necessary along the way. Capturing that additional complexity required a particular kind of contextualizing, which the personally grounded narratives that inform parts of my book were conceived in order to exemplify. If at one level the writing of an intellectual history of contemporary historiography relies on the published traces of explicit debate around which the key developments have crystallized, for example, then at another level it is important to reenter the messier and more elaborate histories that the production of those recognized texts necessarily presupposed—and then helped to move on. To that end I tried to devise a complex framework that honored two distinct but complementary priorities: on the one hand, a close attention to the reading and explication of texts as such (all the pertinent bibliography of the linguistic and cultural turns); but on the other hand, an equally searching analysis of the social circumstances of their production, including the philosophical underpinnings of the relevant historiographical practices, the very specific institutional histories involved, and

sterdam, 2000), 93–109; "Problems with Culture: German History after the Linguistic Turn," *Central European History* 31 (1998), 197–227. See also the earlier, especially thoughtful commentary by David D. Roberts, *Nothing but History: Reconstruction and Extremity after Metaphysics* (Berkeley, Calif., 1995).

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the range of political dynamics that may have impinged. Both of these purposes are vital to the fullest historiographical understanding, I want to argue—that is, *both* the careful and critical exegesis of the major texts themselves *and* the exploring of the dense thickets of discussion and practice through which those texts had eventually been produced. That second context of analysis might be called the profane and imperfect world of historiography.[2]

While confined to certain nuts and bolts of narrative reconstruction—the story of who said what to whom and when—the kind of analysis I am suggesting might easily seem slightly banal. Yet by providing a much fuller account of the sites and settings of intellectual debates, especially within their appropriate institutional and eventful contexts, we can get a much clearer idea of the stakes involved. Such an account would allow us to explore the local and mundane microdynamics of the processes through which ideas become formed together with their wider cultural politics. That in turn can deliver a more complicated and unexpected set of genealogies for the historiographical present. To take a small example, we would understand far more about the enabling consequences and possible limitations of Joan Scott's embrace of poststructuralism in the early 1980s if we knew more about the detailed trajectory that ran from her *Glassworkers of Carmaux* in 1974 through *Women, Work, and Family* in 1978 to *Gender and the Politics of History* ten years later. Scott's transition from teaching women's history at the University of North Carolina at Chapel Hill to directing the Pembroke Center for Teaching and Research on Women at Brown University would be especially interesting, not least in relation to the influence of Denise Riley, who collaborated with Scott during the early 1980s, and whose book *"Am I That Name?"* was one of the emblematic texts I chose in order to characterize the passage to the cultural turn.[3] Through Riley we can then loop back into the transatlantic context of British feminist theoretical debates of the later 1970s. It would be wrong, I think, to disregard these details as mere "storytelling" as opposed to an essential aspect of a careful sociology of knowledge. Such explorations do not diminish the importance of theoretical commentary but rather supplement and thicken it. Some modest pooling of historians' own experiences in these regards, less as autobiography (though that too might have its interest) than as autocritique, could be very illuminating. Part of my intention in offering elements of my own story was to tempt others into doing exactly that, a wish answered by each of my three commentators in their respective ways.

[2] Here my thinking has something in common with Gabrielle M. Spiegel's concept of the "social logic of the text." See her essay "France for Belgium," in Laura Lee Downs and Stéphane Gerson, eds., *Why France? American Historians Reflect on an Enduring Fascination* (Ithaca, N.Y., 2007), 97: "This concept seeks to combine in a single but complex framework an analysis of a text's social site—both as a product of a particular social world and as an agent at work in that world—and its discursive character as 'logos,' that is, as a literary artifact composed of language and thus demanding literary (formal) analysis." See also Spiegel, "Revising the Past/Revisiting the Present: How Change Happens in Historiography," *History and Theory* 46 (2007): 1–19.

[3] See Denise Riley, *"Am I That Name?" Feminism and the Category of "Women" in History* (Minneapolis, 1988). For Scott's three works, see Joan W. Scott, *The Glassworkers of Carmaux: French Craftsmen and Political Action in a Nineteenth-Century City* (Cambridge, Mass., 1974); Louise Tilly and Scott, *Women, Work, and Family* (New York, 1978); Scott, *Gender and the Politics of History* (New York, 1988). Elaine Abelson, David Abraham, and Marjorie Murphy, "Interview with Joan Scott," *Radical History Review* 45 (1989): 40–59, is fascinating on the early years but silent on the 1980s.

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MORE DIRECTLY IN RESPONSE TO THESE THREE COMMENTS, I have several points to make. With respect to the particularities of my standpoint (and some of the attendant points of neglect), I certainly accept Gabrielle Spiegel's deepening of the specifically French background to the intellectual departures of the 1970s. As she points out, both structuralism and poststructuralism in their day had been "intellectually motivated by a rejection of phenomenology," which in Michel Foucault's case extended back through the 1950s to his attendance at Maurice Merleau-Ponty's lectures in the late 1940s.[4] This clearly has a bearing on how we understand both the early critical impetus behind much of the linguistic turn and the current range of second thoughts whose implications Spiegel lays out so acutely. On the other hand, I would argue that the wider, extremely disorderly Anglo-American appropriations of "ideas from France" during the 1970s and 1980s bore an eclectic and uncertain relationship to that more bounded ground of disputation, just as Spiegel's very interesting advocacy of a "neo-phenomenological" direction for the emergent "post–linguistic turn" revisionisms probably proposes far greater coherence than those new critiques can actually bear.[5] In each case, it seems to me, the discursive heterogeneity exceeds the more tightly drawn account that she wishes to provide.[6] Likewise, I readily acknowledge Manu Goswami's observation that I neglect the degree of epistemological indebtedness of the 1980s departures "to the reception of structuralist semiotics . . . from France in the early 1970s" and "the extension of post-Saussurean semiotic models in historical and sociological research." In fact, I can happily confirm this with some testimony of my own: my encounter with a then severely structuralist Marxism on arriving in Cambridge in January 1975 condemned me to many hours of dogged self-education with that particular body of theory. In my book's discussion of what it meant to "think like a Marxist," I gesture toward that extraordinarily intense period of reading and thought (e.g., 18), but it was in the interests of not becoming too autobiographical that I decided to leave much of the detail out.[7]

[4] See David Macey, *The Lives of Michel Foucault: A Biography* (New York, 1995), 31–36.

[5] I am taking the phrase "ideas from France" from the title of one of the emblematic volumes that critically survey the impact of French theory across the academic disciplines and the arts in Britain, originating in a discussion series called "French Legacies" held in November–December 1984 at the Institute of Contemporary Arts to mark the occasion of Foucault's death. It was followed by a conference in 1985 called "Crossing the Channel." See Lisa Appignanesi, ed., *Ideas from France: The Legacy of French Theory—ICA Documents* (London, 1989).

[6] On another front, I do not think that I blur distinctions and "perpetuate . . . confusion by equating cultural history with the linguistic turn" (Spiegel); rather, I took some pains in making exactly that same point. This is what I actually say on the page Spiegel cites: "Writing the . . . history of that extraordinarily complex intellectual upheaval—in a manner commensurate with all its unevenness and diversity and with all the broader cultural, social, and political forces partially explaining it—has so far eluded most commentators. It becomes ever clearer that the favored shorthand descriptions—'cultural turn,' 'linguistic turn,' and 'postmodernism'—were coined in the heat of relatively short-lived, but extremely polarizing, initial battles, disguise as much as they clarify, and conflate manifold variations . . . Turning to 'culture' was the rather vague common denominator for heterogeneous discontents." See Geoff Eley, *A Crooked Line: From Cultural History to the History of Society* (Ann Arbor, Mich., 2005), 156. For an attempt to construct a careful genealogy for the various uses of "postmodernism" by historians during the 1980s and 1990s, see Geoff Eley and Keith Nield, *The Future of Class in History: What's Left of the Social?* (Ann Arbor, Mich., 2007), 57–80.

[7] On another specific note, while I accept the spirit of William Sewell's particular observation, I am not sure that it is fair to call my approach "Anglo-centric" in general. After all, the discussion of social history's genealogies in chapter 2 ("Three Sources of Social History," 25–47) is evenly distributed between British Marxist historiography, French *Annales*, and U.S. social science history; chapter 3 is devoted to German historiography; and the long chapter 4 is located principally in the United States, while

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On a broader front, William Sewell takes me to task for ignoring "the macrosocial environment in which the historiographical changes took place." While this seems overdrawn—the destructive social and political fallout from capitalist restructuring is actually an essential referent for how the book tries to deal with the complicated back-and-forth between politics and historiography[8]—it is certainly true that I abstain from any extensive discussion of either of the two big conjunctures in question, the postwar capitalist boom or the post-Fordist transition that followed. But that was deliberate. My *Crooked Line* was a very particular project: it tried to model a different way of writing about the intellectual history of the present by examining the three-way reciprocities among the historiographical (or more broadly intellectual), the political, and the personal. If the contemporary transformations of capitalism were explicitly coded into my basic understanding of what the category of the political entailed, then for reasons of writing strategy and space, I consciously stayed away from the detailed sociology of knowledge that Sewell wished I had supplied. Although *A Crooked Line* was originally meant to be quite short, more of an extended essay, it graduated during the actual writing into a full-sized book, elaborately equipped with what became a very large endnote apparatus. If in addition I had then tried to undertake the careful and elaborate analysis needed to relate "the epistemic practices of historians" convincingly to the post-Fordist "transformation of the social forms of world capitalism," the scale of that discussion would surely have exceeded the book's realistic purposes.[9] As it happens, my own discussion of those issues can be found elsewhere, partly in my history of the Left in Europe, *Forging Democracy* (the last part of which seeks to address the period since the 1960s in the way Sewell suggests), but especially in my jointly authored book with Keith Nield, *The Future of Class in History*, which was conceived very much as a companion to *A Crooked Line* and directly addresses the relationship between historiography and the real worlds of capitalism.[10]

ranging widely across national and regional historiographies, including substantively crucial treatments of the historiography of race in the U.S. and the subaltern studies school of South Asia. If the three emblematic historians chosen to close each of the main chapters were British (Edward Thompson, Tim Mason, Carolyn Steedman), they have each been hugely influential internationally in their respective ways. It was precisely that duality—their place for my own intellectual formation, combined with their patently non-parochial and transnational resonance—that led me to choose them. So my account seeks to build from its British starting points without being confined by them.

[8] E.g., *A Crooked Line*, 187: "the dispiriting political experiences associated with the crisis of a class-centered socialist tradition from the late 1970s, under the combined effects of capitalist restructuring, deindustrialization, class recomposition, and right-wing political assaults, have profoundly shaped how I'm able to think about the kinds of history I do. For me, the cultural turn was appealing because its implications translated across these different sites—not only my teaching and writing, but also my political knowledge and social understanding, including the everyday settings of personal life."

[9] Again, I *do* explicitly acknowledge the desirability of such analysis. E.g., *A Crooked Line*, 277, note 6: "In my own view, that crisis of 'class-political understanding' bespoke an actually occurring socio-political transition of genuinely epochal dimensions. In other words, together with the larger political and theoretical rethinking it connoted, the cultural turn represented a necessary struggling with contemporary problems, for which the loyal reaffirming of classical materialist positions afforded little help."

[10] In other words, Sewell's "macrosocial" analytic is essential for many kinds of problems, but not for the kind of history I wanted to write in this book. To make connections between historiographical change and capitalist restructuring convincingly would be an entire project in itself. It would certainly require more than any brief and incidental treatment in a book essentially focused elsewhere. For examples of my own macrosocial engagement, see Geoff Eley, *Forging Democracy: The History of the Left*

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However, as well as observing those practical constraints, I had a principled reason for not wanting to conduct a "macrosocial" analysis, too. The clue here can be found in my book's opening treatment of the ways in which Raymond Williams and other thinkers of the later 1960s began calling the prevailing Marxist idiom of "base and superstructure" into question: this initiated a decades-long wrestling with issues of socially materialist explanation, which supplied the red thread for my book's worrying of the relationship between the social and the cultural. Given the cautionary fracturing of confidence in the validity of social analysis, and the resulting antireductionist reticence that I broadly share, the very project of trying to connect extremely particular changes of academic life to "the changing macrosocial forms and fortunes of world capitalism" has become a far more difficult one than Sewell seems to allow. For certain abstract or strategic purposes, it may be plausible to find "a certain elective affinity" between the "general epistemic uncertainty" observable "all across the human sciences" in the late twentieth century and the "heightened 'flexibility' that is one of the hallmarks of the new global economic order," just as the earlier but now "reified categories of the previous intellectual era" may have corresponded to the preceding Fordist regime of regulation. Carefully done, such a sociology of knowledge under the sign of the post-Fordist transition is one I can also find appealing. My *Crooked Line* gestures several times in that direction.[11] But arguments about "the condition of postmodernity" or the "cultural logic of late capitalism" work best at a certain level of abstraction or theoretical generality.[12] The language of "homologies," "affinities," and "correspondences" brings us only so far. In order to show the explanatory importance of changes in the material worlds of capitalism for shifts of interest in an academic discipline, changing paradigms in a field of knowledge, or radical departures in the intellectual practices of historians, something more may be needed. My own argument in *A Crooked Line* was that the aggregation of developments that coalesced into what we call the cultural turn occurred in extraordinarily disparate ways, field by field and institution by institution, with widely varying local histories, and according to very specific and complicated temporalities. To capture those complexities, a particular method of detailed reconstruction would be needed.

Much would depend, then, on how Sewell's "macrosocial" perspective, or what he calls the "deeper social causes," can be shown to have translated into forms of action or patterns of thought on the ground, whether among intellectuals working

in Europe, 1850–2000 (New York, 2002), 337–490; and Eley and Nield, *The Future of Class in History*, preceded by Eley and Nield, "Farewell to the Working Class?" *International Working-Class and Labor History* 57 (Spring 2000): 1–30.

[11] E.g., 151–152, 188–189, 96–97. For Sewell's more detailed explication, see William H. Sewell, Jr., "The Political Unconscious of Social and Cultural History, or, Confessions of a Former Quantitative Historian," in Sewell, *Logics of History: Social Theory and Social Transformation* (Chicago, 2005), 53–63. See also the essays of George Steinmetz: "Scientific Authority and the Transition to Post-Fordism: The Plausibility of Positivism in U.S. Sociology since 1945," in Steinmetz, ed., *The Politics of Method in the Human Sciences: Positivism and Its Epistemological Others* (Durham, N.C., 2005), 275–323; "The Epistemological Unconscious of U.S. Sociology and the Transition to Post-Fordism: The Case of Historical Sociology," in Julia Adams, Elisabeth S. Clemens, and Ann Shola Orloff, eds., *Remaking Modernity: Politics, History, and Sociology* (Durham, N.C., 2005), 109–157; "Regulation Theory, Post-Marxism, and the New Social Movements," *Comparative Studies in Society and History* 36 (1994): 176–212.

[12] See David Harvey, *The Condition of Postmodernity* (Oxford, 1989); and Fredric Jameson, *Postmodernism; or, The Cultural Logic of Late Capitalism* (Durham, N.C., 1991).

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at universities or for other social categories of actors. In that regard, Sewell wishes to link the rise of social history in the 1960s to the post-1945 prevalence in the West of "so-called 'Fordist' or state-centered capitalism." In his view, "the epistemological optimism of social history—its faith in the possibility of reconstructing a history of the social totality—was made plausible in large part by the specific form of capitalist development that characterized the great worldwide postwar capitalist boom." Again, I broadly share this view that "the 'structure of experience' generated by postwar capitalism underwrote the plausibility of social history, whether in its Marxist, its *Annaliste*, or its social-scientific form." Yet I would argue that the primary context for understanding the social history wave in this way was provided less by the distinctive patterns of Fordist accumulation per se, however convincingly their effects can be shown in the expansion of higher education and other ways. Rather, the intelligibility of social history as a knowledge formation, as well as its appeal as a body of intellectual practice linked to wider sociopolitical aspirations, owed far more to the political legacies of 1945 and the distinctive public culture of the long postwar, which were in turn linked to the emancipating possibilities of upward mobility and to a democratic and broadly egalitarian ethic of social improvement. In other words, between the shifting configurations of the "endless accumulation of capital" and their specific effects in politics, culture, and ideas, a great deal of mediation and articulation had to occur—whether those effects concerned the assembling of organized agency inside the polity, or the appeal of social history to particular generations of students in universities.

Thus I do not quite understand why Sewell wants to set up "class" and the "endless accumulation of capital" in opposition to one other, or why it should be necessary to see one as a more "fundamental category" than the other. There are hints here of that old "final instance" syndrome of structural causality, whose implications shadow my own reluctance to undertake the sort of macrosocial explanatory analysis for changes in the ideas of historians that Sewell recommends. To the political and cultural legacies of 1945, as Sewell acknowledges, were also linked extraordinarily successful and resilient traditions of class-centered collectivist politics, whose cohesion and purchase began falling apart during the 1960s and 1970s under the impact of what we would both call the post-Fordist transition. But it is surely hard to abstract from my book's intentionally brief remarks any lack of interest on my part in the overall structure of capitalism. Conversely, I am not sure how else Sewell can conceptualize the possibilities for politics than by the forms of collective agency for which concepts of class remain one necessary means of theorizing. If I emphasize the importance of class in this context, that does not mean that I attach any less efficacy to the "endless accumulation of capital" and its shifting "configurations of political power, spatial relations, class struggles, intellectual forms, technology, and systems of economic regulation." Rather, it seems to me that *both* grounds become essential to any viable framework of analysis on the societal scale: not only the reconfigurations of capitalism as Sewell presents them, but also the bases of organized action and collective agency inside society, for which the changing dynamics of class formation supply one key place from which to start. As Nield and I argue in *The Future of Class in History*, our understanding of all the ways in which class is currently

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being remade will be vital to how effectively we will be able to live within capitalism's latest social imaginary.

There is a more basic issue here. My commentators rightly focus on my insistence that "between social history and cultural history, there is really no need to choose" (181). That insistence had two purposes: it was strategic, in that I argued for the possibility of fruitful conversation across sometimes irreducible yet mutually respectful differences; and it was more specifically historiographical, in that I marked the growing success of historical work that transcends the previous demarcations and sub-specialisms of the discipline. I used the term "new hybridities" to describe the resulting forms.[13] Neither Sewell nor Spiegel likes this stance. They see my unwillingness to choose as a lack of clarity, a "weak" or poorly developed grasp of theory, a lack of ambition, and a falling back into the practicing historian's bad old habits of trusting the virtues of imaginative empirical work and simply "muddling through." Sewell then casts his own proposals in a manner that makes the choice pretty clear after all: facing up to theoretical difficulties versus avoiding them; doing the hard work of theory versus merely striking an attitude; and continuing to take theory seriously versus "set[ting] theory aside and get[ting] on with a wide range of interesting empirical work." But the alternatives are not "theory" versus "no theory," I have to insist. My conclusion may not lay out a fully elaborated theoretical perspective of the kind that Sewell wants to see, and which the final chapter of his own recent book supplies so impressively. But inside the many particularities of its discussions, *A Crooked Line* is nonetheless saturated with theory. My decision not to pull this together programmatically at the end of the book as a clarion call for the future was deliberate. Partly I wanted not to overburden the text. Partly I wanted to observe my conciliationist purpose as mentioned above—the desire, that is, to abstain from insisting on the superior virtues of "one authorizing form of theory against another." I certainly have my own preferences, which so far from being weakly developed are laid out pretty clearly in my book. But I do also think, as a matter of principle, that no one set of theories and methodologies can serve as an answer to each and every question that historians are now trying to ask.

That is what I meant in the book by the need for a "basic pluralism"—in other words, not exactly the wishy-washy eclecticism and fudging of theoretical debates that Gabrielle Spiegel worries about, but rather an acknowledgment that there are different ways of understanding the world, none sufficient in itself for every possible analytical or interpretive purpose. Of course, there is always a biographical dimension to this, too. A huge amount rests on our contingencies of origin, generation,

[13] *A Crooked Line*, 201. Whereas my own statement took pleasure in the growing redundancy of the discipline's traditional divisions between discrete regions of study (such as social history and cultural history), Sewell took me to mean that "hybridity" was the decisive and praiseworthy characteristic specifically of the "new cultural history." But this is what I actually wrote: By the 1990s, many former social historians "now moved increasingly freely across the old distinctions between the social, the cultural, the political, the intellectual, and so forth, allowing new hybridities to form." The difference between our two renditions is quite significant. By hybridity in this context I meant studies that "specifically refuse the polarized division between the 'social' and the 'cultural,' vesting recognizably social and political topics with a cultural analytic, responding to the incitements of cultural theory, and grounding these in as dense and imaginative a range of sources and interpretive contexts as possible." In that sense I argued that the categorical opposition between "social" and "cultural" should be seen as unnecessary and mistaken.

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schooling, cultural formation, political conjuncture, and so forth. We each enter the stream or get on the train at different points along the way, usually with differing destinations in mind (or different imaginings of what the destination might look like), having missed earlier stretches of the journey that for others turned out to be the really formative part. Spiegel's explication of the phenomenological backstory to the structuralist Marxist departures of the 1960s and 1970s (which is where my own biography jumped on the train) is extremely helpful. So too is her very acute characterization of what many current commentators are seeking to come up with in response to the consequences and insufficiencies of the cultural turn. Yet, understandably enough, her actor-centered perspective and neo-phenomenological approach seem best fitted for her own preferred kind of history—mainly intellectual and cultural, less concerned with (for example) state formation, social movements, the politics of collective action, and other areas where another set of approaches might be deployed. Her proposal offers much less help for William Sewell's macrosocial problematic or for meeting Manu Goswami's call for new histories of comparative capitalism. But we each have our own likings; and for varying kinds of questions, different kinds of theory will be called.[14] Who is to say, really, that one tradition is "better" than another? Frankfurt School as opposed to post-Althusserian Marxism? Gramsci as opposed to phenomenology? Habermas rather than Foucault? In that form, the questions are absurd. I have my own very strong preferences for certain kinds of theory over others, relative to the kinds of questions I mainly want to ask. But the fact that some of my colleagues hold equally coherent but differing views for different types of questions ought not to be an impediment to important forms of collaboration. I cannot imagine that either Spiegel or Sewell would disagree, and yet these are the terms in which my plea was made: mutually respectful coexistence, the building of coalitions, reasoned intellectual exchange.

So Spiegel is right to wonder whether methodological and epistemological pluralism can be "a genuinely theoretical position." But in proposing it, I was not trying to duck the hard choices. Nor was it the kind of eclecticism that wants blithely to give everyone their own thing. My *Crooked Line*'s "basic pluralism" was more about the politics of intellectual collaboration through respect for differences—differences of theoretical outlook, differences of epistemology, differences of intellectual biography, differences of project. However rigorously we explicate our own positions, those differences will in any case always remain. Some extraordinarily original and intelligent thinkers among our colleagues may want to pursue projects through modes of inquiry using methodological approaches and theoretical frameworks that we find profoundly wrongheaded, theoretically antithetical to our own preferred ground, and epistemologically flawed. But that does not necessarily mean that we cannot enter into fruitful conversation, that we cannot form important intellectual collaborations at a variety of institutional and intellectual levels, and that we cannot find common enemies. Nor, it should immediately be conceded, does that absolve

[14] Likewise, those preferences can make us less patient with the grounds from which others do their different kinds of work. Thus it is simply not the case (as Spiegel claims) that either an "empirically grounded social history" or an acknowledgment of the importance of structures "implicitly reverts to that 'noble dream' of an objective basis for historical investigation."

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us from the need to be clear and consistent in the standpoints we prefer for ourselves.[15] Martin Jay expresses the principle involved with characteristic generosity:

> any given analysis, if it is sincerely pursued, produces the necessity of its alternatives. Every particular approach, whether it is hermeneutic or theoretical or narrative or experiential, will be ultimately inadequate. You will reach a point where it will fail to do justice to the complexity of the phenomena. What is really required is a nimble way to move from one mode of analysis to another without expecting them to necessarily cohere in a definitive way. In this sense, the image of a force-field or constellation, which I've always found so useful in my work, captures the inexhaustible variety of our interface with the world better than any single unified approach.[16]

FINALLY, IF I HAVE LESS TO SAY ABOUT MANU GOSWAMI'S COMMENT, it is because I share her sense of the foreshortening in the new cultural history's seeming incapacity for "future-oriented" thinking—that is, its apparent reluctance to replicate social history's earlier commitment to a vision of social transformation that could both frame the effort at appropriating the past and open up "the prospect of a radically other future." Such a vision would need to contain "the potential of mediating between the past and the present, the actual and the possible." This is congruent with what I argue at the very beginning and very end of my book. But if my own discussion emphasized more the "space of experience," Goswami rightly asserts the importance of the "horizon of expectation," too (to invoke Reinhart Koselleck's conceptual couplet), regretting that cultural historians seem so uninterested in shaping their projects by means of a radically transformative conception of a future lying anywhere beyond the current hegemonies of a permanently unfolding neoliberal present.[17] Much of that critique seems justified. I can certainly see how "the organizing premises of cultural history," which Goswami lists as "contingency, episodic ruptures unmoored in immanent social contradictions, undecidability, difference, fragmentation," can encourage a backing away from "whole society" analysis. As *Crooked Line* argues, this expressed not just a pragmatic adjustment to the historical present, but often a conscious choice in a time of terrible defeat and disappointment, when the demonstrated exhaustion and destructiveness of large-scale projects of remaking the social world seemed to make small-scale reconstructions the only viable ethical course.[18] As Goswami says, social history and cultural history sustained distinct

[15] And it certainly does not imply an aversion to conflict, any blanket opposition to the fruitfulness of polemical exchange, or an unwillingness to confront genuine differences and disagreements when they appear. They are limits to the possible collaborations, too.

[16] Douglas J. Goodman, "Dream Kitsch and the Debris of History: An Interview with Martin Jay," *Journal of Consumer Culture* 3 (2003): 119.

[17] See Reinhart Koselleck, " 'Space of Experience' and "Horizon of Expectation': Two Historical Categories," in Koselleck, *Futures Past: On the Semantics of Historical Time* (Cambridge, Mass., 1985), 255–276; also David Scott, *Conscripts of Modernity: The Tragedy of Colonial Enlightenment* (Durham, N.C., 2004), 23–57.

[18] I should say that here I am speaking quite specifically about the Left experience in the Euro–North American West, a delimitation that was also repeatedly and explicitly marked throughout the text of *A Crooked Line*. Likewise, I see the turning away from "whole society" analysis during the 1980s as specifically characterizing historians who are working in and on the West, although it certainly has its analogues elsewhere. I made a point of emphasizing these particularities in the book, deliberately ex-

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forms of political subjectivity in that sense. I can see the force of her more specific critique of the "post-Gramscian turn" in subaltern studies, too.

These aspects of Goswami's extremely clear-sighted discussion bring me back to the starting point of this response. For if I remain optimistic about the chances of reactivating certain kinds of conversation beyond the divisiveness of earlier historiographical differences, then my sense of present political endangerment also keeps its urgency—the sense, that is, of no longer being sure where the grounds of any future-oriented optimism about the directions of change in the actually existing worlds of capitalism might still be found. Thus for me, part of the intellectual history of the present has to involve not only a degree of political self-consciousness in all the ways alluded to above, but also a willingness to explore the material or structural conditions of possibility that help make sense of such a generalized turning to cultural history across so many different fields during the past two decades in the first place. In those terms I am definitely sympathetic to some version of the project that Sewell has described. Otherwise, as Goswami compellingly argues, cultural history can offer only an incomplete account "of its own global emergence and resonance." However mediated and complex, "only a framework attuned to the dynamic interchange between social transformations and categories of perception" can help ground the accomplishments of cultural history in the most generative of ways. Yet it is only *now*, and precisely *because* the cultural turn has been accomplished, that we can begin to bring these broader contextual fields into focus—those "new histories of society" I called for at the end of my book, or those "forms of historical totalization" that Goswami describes cultural historians as having first needed to reject.[19] If we write our own histories, it might be said, then we do so with the benefit of theoretical approaches, types of methodology, and general historiographical supports that are not always of our own choosing. Moreover, we set ourselves only the questions that we know need to be answered.

Yet I would not agree that the generality of cultural historians "either overtly celebrate or uncritically reflect the absence of a systemic alternative to the predominant social and cultural forms of the present." Even if the cultural turn can sometimes impede the writing of new histories of society, to speak baldly of "the corral of culturalism" goes for me a polemic too far. The nuances are important. I did not intend either a "celebration of contemporary cultural history" or its blanket "endorsement." But nor do I see the terrain left by the cultural turn as merely a "shapeless bazaar" where younger historians are able simply to "muddle through" (Sewell).[20] As my book set out to argue, the turning *away* from Marxisms, materialisms, and macro-historical ambitions registered a real crisis in those given forms of understanding, just as the turning *toward* culturalisms of various kinds enabled some real gains and solutions. If I say that social history "simply isn't available any more," I am referring obviously to "social history" "in the form of the original project," and that does not mean that I can see no recuperable forms of social anal-

empting works written from "an extra-European vantage-point" (197). But I very much welcome Goswami's further reminder.

[19] For my own recent attempt to engage this project, see Geoff Eley, "Historicizing the Global, Politicizing Capital: Giving the Present a Name," *History Workshop Journal* 63 (Spring 2007): 154–188.

[20] Sewell's remark does younger historians a serious injustice.

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ysis.[21] Divested of the earlier expectations, while taking on board all we can learn via the cultural turn, new histories of the social will certainly be written. But likewise, if I uphold the gains that cultural history enabled, in the ways I used Carolyn Steedman's work to exemplify, that does not mean I believe that everything about it can be "uncritically" accepted. Let us sit down together and think about the various ways, some large and some small, in which these differing projects might fruitfully be joined. In the interests of *that* conversation, it is precisely important that we not be required to choose.

[21] See *A Crooked Line*, 189. I then spell out the terms of that original project: "Its coherence derived from the sovereignty of social determinations within a self-confident materialist paradigm of social totality, grounded in the primacy of class."

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Featured Reviews

HAROLD J. COOK. *Matters of Exchange: Commerce, Medicine, and Science in the Dutch Golden Age*. New Haven, Conn.: Yale University Press. 2007. Pp. xiv, 562. $35.00.

ANNE GOLDGAR. *Tulipmania: Money, Honor, and Knowledge in the Dutch Golden Age*. Chicago: University of Chicago Press. 2007. Pp. xx, 425. $30.00.

In 1593 the doyen of European naturalists, Carolus Clusius (Charles de l'Escluse), was enticed to leave his post at the imperial court of Vienna to become the founding director of Leiden University's Hortus Botanicus. In doing so Clusius was returning, after a life of extensive travels—he was then sixty-seven—to his native Low Countries, although the Dutch Republic, a Protestant church, and the university at Leiden had not existed during his youth in Arras. Clusius was a formidable collector and an avid correspondent. He formed the center of a network of naturalists, gardeners, and collectors through which rich information circulated. The new botanical garden in Leiden, supported by well-connected patrons, gave that network an institutional embodiment. At Clusius's death in 1608, the learned jurist Justus Lipsius described him as "the father of all the beautiful gardens of this land."

Both of the books under review here launch their very different agendas with accounts of Clusius and his work. He stood at the center of the circle of *liefhebbers* (enthusiasts, connoisseurs) who disseminated the tulip in the Dutch Republic and nurtured knowledge of and stimulated a remarkable enthusiasm for this remarkable root. Clusius's interests extended beyond tulips, of course, and his foundational role in the natural sciences serves as a launching pad for Harold J. Cook's study, which offers a broad-ranging account of the networks of collectors, empiricists, and medical doctors who, in Cook's view, cumulatively established medicine and natural history "as the big science of the early modern period" (p. 410).

Clusius and his circle of *liefhebbers* also found practical outlets for their enthusiasms. They cultivated tastes as they cultivated gardens, and as the tulip became an object of desire as well as an object of curiosity, it became apparent that "commodification" would be an inevitable part of the experience of being a *liefhebber*. This insight sets the stage for Anne Goldgar's deeply researched investigations into the world of the tulip fancier, where the great goal, of course, is to understand the tulip mania of 1636–1637.

So, one book is about science and the other is about commerce. But both authors see immediately that matters in the early modern era could not be so simple. For Goldgar, the trade in tulips can only be understood in a broad, interdisciplinary context: the botanical, art historical, and commercial aspects of her subject must be embedded in the social relations and interactions of the communities that came into contact with the tulip. Cook, too, insists at the outset that his aim is to show that early modern people lived "united lives": that their intellectual interests were deeply embedded in their social, political, and economic activities. More specifically, his central argument is that the commercial way of life—its empirically based practices, its focus on fact, observation, and experience—profoundly shaped the character of scientific inquiry in the Dutch Republic. Both authors fly the flags of interdisciplinarity and wholism.

Goldgar's brave and remarkable book tackles what, for foreigners, is arguably the best-known event of Dutch history. The story has been retold over and over by historians, economists, and journalists, always relying heavily on the spirited account of Charles Mackay in his classic 1841 study of aberrant behavior in financial markets, *Extraordinary Popular Delusions and the Madness of Crowds*. But what did any of these writers actually know about Dutch tulip markets? Goldgar followed the references with care, and they led her always to a small number of illustrated pamphlets written in the immediate aftermath of the tulip bubble that aimed to ridicule the participants in the bubble and admonish readers to avoid a repetition. These few pamphlets, together with a small number of legal documents pertain-

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ing to tulip traders, were gathered together and published in the 1920s by the eminent Dutch economic historian N. W. Posthumus. On this miniscule and far from value-free foundation rests the entire corpus of tulip-mania writings. It was, it seems, too good a story to resist, and much too good a story to subject to historical source criticism. After all, even modern peoples need their legends. But Goldgar has burst the mythic bubble. For the first time we now have a substantial number of new archival documents that shed direct light on the tulip trade and the bubble, and an informal, critical reading of the existing materials. She dedicates her book to the memory of Michael Montias, the economist whose interest in Dutch art led him to the archives of Delft, where he found document after document that no "specialist" supposed existed on the lives and careers of artists, including Jan Vermeer. Like Montias, Goldgar has immersed herself in Dutch municipal archives, where she uncovered information on 370 tulip buyers and/or sellers in Haarlem (the center of the trade), Amsterdam, and Enkhuizen. Goldgar not only presents this new evidence; she also corrects the previous record, undoing many of the errors introduced by the editing work of Posthumus.

Armed with data drawn primarily from the rich notarial archives and the records of the Haarlem small claims court, Goldgar leads her readers via the microhistorical prism of the tulip mania into the Dutch Brave New World. This, she writes, was "a new country, with a new kind of culture, a new set of priorities, a new social system, a new impetus to its commerce and society. Tulipmania opens a window for us into that culture and its value" (p. 7). Goldgar describes the participants in tulip transactions (merchants, innkeepers, brewers, and bakers were well represented, and a surprisingly large number were Mennonites) and identifies the taverns where *floristen* (the participants in the trade, devotés of Flora) gathered to discuss and trade tulips. She delineates the hierarchies among the *floristen*, and the informal order and authority, based on expertise and taste, that governed this community.

But, to cut to the chase, this community's institutional practices could not (if it ever wanted to) prevent a speculative bubble in the tulip trade, and they did not possess the internal cohesion to enforce agreements once the bubble burst on, apparently, February 7, 1637. Contrary to the baseless claims of nearly all writers on this subject, the collapse of the bubble did not lead to widespread bankruptcies, a liquidity crisis, or an economic depression. Goldgar confirms that it left barely a trace in the larger economy and society, but she insists that it left deep scars in the communities of tulip traders, and deep concerns among those who viewed the spectacle from a distance: seekers of beauty and knowledge had entered into a new capitalist world and now confronted broken promises and compromised honor. In this new world who and what might be trusted? What was worthy, what was valuable? This is where Goldgar seeks the "meaning" of the tulip mania, but I believe she misconstrues the nature of the actual consternation faced by these Dutch pioneers in capitalism.

The trade in tulips presented a number of practical difficulties to the *floristen*. Since the bulbs were lifted from their beds in June and replanted by September, they could only be delivered to new owners in the summer months, and since they bloomed only a few weeks in the spring, buyers relied largely on trust (and on catalogue books of illustrated tulips) concerning the authenticity of their purchases. Authenticity was easily contested given the inherent variability of the tulip, which was at once a source of allure and of risk. In short, the trade depended utterly on the authority and probity of the participants and on future delivery and payment of contracted transactions. This last feature led easily to futures trading, where a seller does not (yet) possess the goods being sold (and the buyer may expect to sell again before actually taking delivery). In Goldgar's archival investigations, the longest "chain" of transactions on the same bulb involved five trades between July 1636 and February 1637.

Futures trading was not unknown in the Dutch Republic, but it was illegal in the sense that such transactions could not be enforced in the courts. For this and other practical reasons, *floristen* had to develop trading practices that relied on intragroup enforcement. This sort of informal institutional development is now of great interest to economists and economic historians. Goldgar describes some of the informal group cohesion practices of the *floristen*, but her highly empirical study does not deal conceptually with the organization of the trade. This omission causes her to avoid inquiring into the likely relationship of the notarial and court records she excavated to the larger phenomenon of tulip trading. Is her evidence typical, or does it shed light on a particular, elevated, layer of the trade? More seriously for her interpretation of the tulip mania, she remains puzzled by the stubborn and persisting inaction of every level of government when the bubble burst. In response to petitions from *floristen* to intervene with Solomonic decrees separating the good contracts from the bad, the States of Holland and the High Court of Holland simply urged that town magistrates use their good offices to bring buyers and sellers to voluntary settlements of their disputes. As disputes festered and deepened, the social climate among *floristen* deteriorated. In Goldgar's view, this led despairing Dutchmen to begin to wonder about the nature of value, trust, and honor. But, the problem of asset valuation is with us today just as it was with these capitalist pioneers. The instability in the tulip trade was related to the specifics of that trade, which Goldgar describes in admirable detail; it was not, and was not interpreted as, a metaphor for commercial society as a whole. Goldgar overstates her case.

Nor were the stinging satires ridiculing the *floristen* and their trade a sign of a deep moral crisis of a society grappling with the new dangers of early capitalism. The reason those pamphlets and broadsides could give rise to the durable legend of the tulip mania is that the same messages have found receptive audiences right up to

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the present day. We are, presumably, well accustomed to capitalism and its surprises, yet we continue to feel a strong need for selective expressions of contempt for greed and avarice; even in a secular age we are happy to believe that a just God will chastise speculators and fearful that He may also extend his displeasure to our TIAA-CREF and 403(b) accounts.

Goldgar's book establishes a new benchmark—the first since 1637—for interpretations of the tulip mania. It largely fulfills its ambitious interdisciplinary agenda, bringing to life the world of the seventeenth-century *floristen*. But her failure fully to analyze the institutional weaknesses of the trade leads Goldgar to conclusions that overstate the novelty and meaning of the moral distress brought about by the collapse of the tulip trade. The gap between the bubbles of 1637 and 2000 is, I suspect, not as great as she claims, but with this book readers for the first time have much more information with which to decide for themselves.

When Clusius surveyed his collection of tulip bulbs, he was well aware that they had a monetary value—theft from his gardens was an ongoing concern—but he valued them primarily for their botanical interest and beauty. However, Goldgar shows that for most *liefhebbers* scientific value, aesthetic value, and monetary value were inextricably connected. The trade in tulips did not stand in opposition to the connoisseurship of tulips: the two went hand in hand. The central argument of Cook's book is that the commercial spirit of Dutch society imparted a vital element to its scientific pursuits. The values of observation, accumulation, accounting, and credible fact inherent in the market spilled over to the work of naturalists of all kinds, allowing them to observe, collect, and "know" the natural world (for the first time?) unhampered by the iron fetters of speculative thought.

For historians of science these are fighting words. By arguing for the centrality of an observation-based natural philosophy as developed in the seventeenth-century Netherlands, Cook is seeking to move both the Netherlands and the study of medicine and natural history from the margins to the center of the narrative of the early modern history of science.

Dutch scientists from Clusius (1526–1608) to Hermann Boerhaave (1668–1738) have been praised for their meticulous description of nature, but this praise has generally been tempered with the condescension commonly felt by theoretical science, a science of causal explanation, for empirical science. Cook's aim in this book is to show that experiential scientists, with their gardens, collections of rarities and grotesqueries, and endless descriptions constituted the "big science of the early modern period" (p. 410). Moreover, for Cook this scientific efflorescence occurred when and where it did for a reason: the great project of discovering objective truth did not occur simply because of "a mental revolution arising from pure thought," but because of the beneficial example of commerce society. Investigating empirical details became satisfying, while they had earlier "been condemned as the cause of error and sin . . . [because] the values of the urban merchants, including their intellectual values, were increasingly dominant throughout society" (p. 411).

How does Cook seek to render his large claims plausible? The bulk of his book consists of a series of narrative accounts of the scientific careers of notable Dutch-based naturalists and medical doctors, emphasizing the socioeconomic context in which they worked, the nature of their scientific achievements, and the networks of scholars, merchants, politicians, and clergy among whom their work circulated, and by whom it was challenged, corrected (or mangled), accepted, and integrated into the scientific understanding of the time. Many scientists, some well known, others familiar only to specialists, pass through the pages of the book. Cook, whose earlier work focused primarily on English medicine, takes evident relish—as much as a seventeenth-century botanist—in presenting his historical specimens for our examination, knowing that the Dutch scientific world he is laying bare will be unfamiliar to most of his readers, just as the Asian and New World *naturalia* assembled and described by many of the Dutch scientists were sources of wonder to their learned audiences. Bernard Paudanus, Jacobus Bontius, Georg Marcgraf, Louis de Bils, Frederick Ruysch, Cornelis Bontekoe, Robert Padtbrugge, Jan Bax, Andreas Cleyer, Hendrik Claudius, and Willem ten Rhijne are among Cook's subjects, along with better-known figures such as Nicolaes Tulp, Casparus Barlaeus, René Descartes, Baruch Spinoza, Jan Swammerdam, Georgius Everhardus Rumphius, Maria Sibylla Merian, Bernard Mandeville, and Boerhaave.

This list, while not exhaustive, suffices to show that Dutch naturalism and medicine in the period 1600–1730 were densely populated with remarkable figures whose work spanned the globe, from Japan and the Moluccan islands to Brazil and South Africa, making the Republic a uniquely information rich environment. Moreover many of these figures were not of Dutch origin, but were drawn into the orbit of Dutch natural philosophy. It was a cosmopolitan scientific community, yet somehow infused with a common Dutch commercial spirit.

Many if not most of Cook's narrations take him far from the Dutch university towns and from the republic itself to the colonial and trading outposts of the Dutch trading companies in both Asia and the New World. An alliance of trading companies and empirical natural science should fit Cook's thesis like a hand in a glove. But does the glove really fit? Just what was the role of the Dutch East India Company (VOC) in the vast and varied scientific work carried out by its servants? At times the company sent out directives, embarking on a sort of bio-prospecting, and it never lost sight of the terrible challenge of keeping its employees alive in the tropics. The botanical gardens on Java and at the Cape gave naturalists in Leiden access to an invaluable resource. But why was a commercial enterprise engaged in scientific research? Or, to turn the question around, what in the company's conduct rubbed off on its servants, equipping them for lives of keen, objective observation?

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Making the jump from correlation (the Dutch Republic was commercial; the Dutch Republic was a center of natural science) to causation (commerce shaped the nature of Dutch science) is never easy for historians, but this study leaves more than the usual gap between its introductory claims and the examples that fill the book. While the vignettes are always interesting, it is not always apparent how they advance the thesis, or, sometimes, why they were selected.

Still, I suspect many readers will be prepared to accept Cook's claims about the alliance of commerce and observational science. But what about religion? Was the rise of science in the Dutch Republic not also the result of Protestantism? Here Cook is as adamant as he is unconvincing. In his view, religion only gets in the way of science: the rise of science in the republic is the result of "the ability to escape the intellectual constraints that religious figures of many kinds wished to place on everyone" (p. 83).

It is not common for a scholar to retool himself in mid-career and plunge seriously into the history of another society, as Cook has done. One runs the risk of misinterpreting things large and small, and this volume has its share of misspellings and—mostly non-fatal—gaffes. On the whole it is a spirited synthesis of a large and rambling literature with a serious, if debatable, thesis. But when Cook turns to religion in the Netherlands, he relies on overdrawn, one-dimensional characterizations of the main theological movements. The orthodox Calvinists are, in his view, determined to stamp out free scientific inquiry while other religious currents he supposes all to have been essentially "libertine" in spirit. By assuming that Remonstrants were really crypto-secularists and that Contra Remonstrants were akin to modern fundamentalists he sets up straw men and avoids a serious investigation of how religion and science interacted in Dutch society. The Calvinists in his account are ever threatening yet never effective in shutting down a science in which he shows they participated fully and in much the same way as adherents of other traditions. The "united lives" Cook wishes to show these early modern scientists leading seem to exclude their religious lives.

The literature on the Dutch golden age is certainly enriched by these two studies. They both deserve to be broadly read—and vigorously debated.

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Jonathan I. Israel. *Enlightenment Contested: Philosophy, Modernity, and the Emancipation of Man 1670–1752*. New York: Oxford University Press. 2006. Pp. xxiv, 983. $55.00.

The weekly French news magazine *Le Point* (the contemporary French equivalent of *Time* or *Newsweek*) recently confirmed for the wider public what those who study modern European intellectual history already know: *Baruch Spinoza est très à la mode*. The July 12, 2007, cover story offered a detailed historical account of Spinoza's life and work, illuminated by sidebars with passages from Spinoza's texts "for meditation" and an analysis of the religious tolerance of Spinoza's native Amsterdam and the philosopher's fraught relationship with Judaism. A seventeenth-century Dutch painting of the Amsterdam stock exchange illustrated the text. Spinoza's singular influence among modern philosophers was also noted through testimonies from Friedrich Nietzsche, Karl Marx, Louis Althusser, Leo Strauss, Jacques Lacan, Antonio Gramsci, Gilles Deleuze, and Antonio Negri. Completing the story was an interview with Pierre-François Moreau, who offered two possible definitions of Spinozism. According to the more rigorous and limited of the two, Spinozism is the philosophical doctrine of Baruch Spinoza as found in his published writing. In the second, Spinozism is not so much a precise philosophy as a broader historical constellation of thought, namely the "eruption (*ébullition*) of radical ideas" that began in Amsterdam in the 1660s and then spread throughout Europe and the world in the process that has since come to be called Enlightenment. Moreau attributed the latter conception to Jonathan Israel's 2001 book *Radical Enlightenment: Philosophy and the Making of Modernity 1650–1750*. The magazine further supported the reference by citing the 2006 French translation of *Radical Enlightenment* published by Editions Amsterdam, a press that bills itself as "fully engaged in the debates of the present" through its work to diffuse "radically democratic and anti-authoritarian thought in step with the present condition." In 2007, Editions Amsterdam also published the papers delivered at a London symposium devoted to Israel's work, one that included scholars from the United States, Britain, Holland, Germany, Italy, Spain, and France (Catherine Secrétan, Tristan Dagron, and Laurent Bove, editors, *Qu'est-ce que les Lumières "radicals"? Libertinage, athéisme et sponizisme*).

Several aspects of the *Le Point* issue speak to the larger themes of this review. First is the fact that it is not a single book under scrutiny, but a larger project that contains this book, its 2001 prequel, and a projected third volume in preparation. Second is the related point that this book and its equally lengthy companion each run to roughly a thousand densely worded pages. They are not simply meticulous works of erudite historical scholarship; they are also *machines de guerre* deployed in a self-conscious program of international scholarly activism. Third is the contemporary global ad-

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dress of Israel's project and the intense international interest that it has so far generated. Fourth is the presence at the work's center of a particular relationship between history and philosophy, one that makes Israel's initiative simultaneously an intervention into the professional academic field of intellectual history and one into the wider philosophical discourse of modernity.

This multidimensional enterprise was launched in *Radical Enlightenment*, and while the second volume embarks on some new trajectories generated by critical reaction to the first, it is largely a sequel that undertakes to deepen, extend, and hone the central argument of the first book. *Enlightenment Contested* also takes it for granted that the foundation for its central argument has already been secured by *Radical Enlightenment*. Accordingly, any understanding of the second book must begin with a recapitulation of the argument of the first.

Israel locates the birth of the Enlightenment singularly in the radical philosophy of Spinoza and sees the spread of Enlightenment in the advocacy and dissemination of radical Spinozist ideas by an activist intellectual underground that spread from the Low Countries throughout Europe after Spinoza's death in 1677. Central to this story, which is told through meticulous reconstruction of the documentary and biographical history of the network, is the historiographical claim that members of this radical Spinozistic coterie should be recognized as the real avatars of Enlightenment, while the family of moderate philosophers and philosophies that has traditionally anchored canonical accounts of the movement should not. With this realignment of lenses, once iconic figures of Enlightenment such as John Locke, Isaac Newton, Voltaire, and David Hume find themselves positioned against the movement, while otherwise obscure figures such as John Toland, Walter Tschirnhaus, and Nicolas Fréret find their importance within it magnified.

Enlightenment Contested takes for granted the Spinozist origins of Enlightenment argued for in *Radical Enlightenment*, and thus begins in the 1680s with the battles provoked by the expanding diffusion of radical Spinozism throughout Europe. It further deploys the analytical terms of the previous work by applying them toward what Israel calls "a controversialist approach to intellectual history." In this approach, the European intellectual debates of the years 1680 to 1750 are reconstituted in detail so as to show how modernity (or "modernity"; Israel has a fascinating tendency to apply scare quotes to this term on some occasions and to avoid them on others, seemingly without rationale) emerged through the assertion by the vanguard "Radical Enlightenment" of Spinoza's belief in "individual liberty, freedom of expression, comprehensive toleration, equality racial and sexual, freedom of lifestyle, full secularization of all legal institutions and most publicly maintained educational establishments, together with a wholly secular morality based on equity." The Radical Enlightenment also fought in the name of Spinoza to defeat "theological criteria, supernatural agency, tradition, magic, and racial and hierarchical conceptions of society." Against them in these efforts stood the "moderate, mainstream Enlightenment" on the one hand (i.e., Locke, Newton, Voltaire, etc.), who accepted only some of these ideas, or who embraced them in a limited or less steadfast way, and the religious, monarchist, and anti-philosophical "Counter-Enlightenment" who rejected Spinoza's philosophical modernity on the other.

So what are we to make of this ever-growing scholarly Leviathan and the media phenomenon that it is producing? The tripartite taxonomy of Radical Enlightenment/Moderate Enlightenment/Counter-Enlightenment constitutes the project's most significant innovation, for with it Israel proposes to redefine the understanding of Enlightenment currently held by academic historians of modern thought. Anthony La Vopa captured well the impact of this intervention among professors of intellectual history when he noted that in the wake of Israel he would need to completely re-conceptualize his lecture course on modern European intellectual history (*Journal of Modern History* 75:2 [2003]: 389–393). *Enlightenment Contested* will aid immensely in this project by offering students of modern thought an astonishingly well-researched and well-documented empirical account of the European philosophical debate between 1650 and 1750.

The trouble is that Israel attempts far more in these works than mere biographical and textual documentation. Moreover, when he reaches beyond the world of academic history of ideas and into the contemporary philosophical discourse of modernity, and when, in his most ambitious moments, he implies that his positivist, empirical history of eighteenth-century philosophical debate should be read as a substantial revision of the philosophical history of modernity as a whole, a host of frustrating problems follow in train. Israel is primarily concerned with the discussions of Enlightenment currently occurring among professional, academic historians, and he frames his intervention in terms of an almost half-century-old conception of the field of intellectual history as divided between idealist "men and ideas" historians of thought and materialist social historians of ideas (i.e., Peter Gay's Enlightenment vs. Roger Darnton's and Roger Chartier's alternatives). Israel proposes that the materialist social historians have ruled the roost for too long, and that fruitful revisions will result from returning the emphasis to the pure history of thought. Sociocultural history has indeed been in the ascendancy for some time among professional academic historians, and this bias has produced a body of scholarship about Enlightenment that suffers from a tendency to evacuate the very philosophical object at issue, Enlightenment modernity, through a focus on only one dimension of it: the material dimension manifest in the commerce of books, the materialities of authorship and reading, market pressures, the new capitalist public. The problem is that despite Israel's zeal on behalf of returning the philosophy back into the history of Enlightenment, he remains a positivist, empirical historian at root. His actual work thus stands up

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much better as an empirical history of the books, authors, and intellectual lives that made the Enlightenment than it does as an intellectual and philosophical history of this movement.

In fact, rather than the return of philosophical intellectual history that this book announces itself to be, *Enlightenment Contested* really offers another kind of social history of ideas, one that is admittedly opposed to deploying any conception of the social as a determinant for philosophical thought, but one that nevertheless shares with the writing that circulates under the label "social history of ideas" a methodology centered on providing positivist accounts of the empirical lives of ideas, books, and authors. Many will no doubt respond by asking: "But what else is intellectual history but this?" Certainly a wider academic community exists sharing Israel's methodological inclinations, yet intellectual history also contains other self-conceptions besides this one. With respect to the history of modern thought, these other approaches especially include those linked more strongly with continental philosophy, cultural studies, anthropology, critical social theory, and other hermeneutic human sciences. Viewed from the perspective of these approaches to intellectual history, *Enlightenment Contested* is best described as an empirical history of eighteenth-century philosophical debate narrated through a resolutely positivist account of the progressive march of Enlightenment modernity through the advance of secularism, atheism, and revolutionary liberty and equality.

The non-reflexive positivism of *Enlightenment Contested* contributes to many of its problems. For Israel, eighteenth-century philosophy exists in a space detached from the historian who writes its history. When writing the history of philosophical modernity, however, this tried and true stance of the positivist historian is not so much a rigorous methodology as its own unrecognized philosophical intervention into the subject. It produces a lifeless reduction of the complex philosophical negotiations at issue to a rigid set of "isms" that can then be narrated according to a mechanical developmental logic. A two-dimensional representation of the philosophers and writers at the center of this history results, since they are not treated as the complex thinkers and actors that they were, but as wooden representatives of the various "isms." The texts they wrote are also treated as dead receptacles of historical thought and not as the living engagements with the ongoing problem of modernity that they in fact are. These books and authors are further subjected to simple judgments that turn them into poster children for the camps in the master Radical Enlightenment/Moderate Enlightenment/Counter-Enlightenment taxonomy. The story that this schematic mechanism produces is frustratingly teleological, with the triumph of secularism, materialism, and atheism, along with liberty, equality, and toleration, serving as the overdetermined outcome of the debates that Israel reconstitutes, and the defeat of mysticism and irrationalism, along with religion, tradition, and hierarchical society as their natural and inevitable consequence. Modernity/"Modernity," the category anchoring the whole enterprise, is likewise never defined. The conceptual aporias attendant to it are also never broached, nor are those of its companion concepts such as secularism, equality, liberty, revolution, rights and its conceptual enemies such as tradition, superstition, and religion. Each of these highly complex and contested terms is in deployed as if each were a self-evident concept with a meaning that can be taken for granted. In tautological fashion, Spinoza and the Radical Enlightenment are also understood to both embody modernity/"modernity" from the start and to make it historically at the same time. Indeed, when reduced to its essential argument, *Enlightenment Contested* really offers a narrative remarkably similar (albeit with a different cast of heroes) to the eighth and ninth epochs of Condorcet's 1795 *Esquisse d'un tableau historique des progrès de l'esprit humain* (*Sketch for a Historical Picture of the Progress of the Human Mind*), the ones where science and philosophy throw off the yoke of priestly and kingly authority so as to prepare the way for the formation of revolutionary republicanism.

Israel's history is indeed the eighteenth-century emancipation of universal man from religious and monarchical tutelage all over again. Yet while Condorcet offered his version of this story as piece of contemporary philosophical and revolutionary activism, Israel, by contrast, positions himself as a positivist historian detached from interested philosophical engagement in the present. The Radical Enlightenment whose history he presents is therefore kept distant from the present that he is addressing with his work. On one side of this chasm is the tradition of radical philosophy that flows out of Spinoza's work. This philosophy flows into the Enlightenment and revolutions of the eighteenth century and on into the nineteenth century where it finds its resting place, to judge from Israel's narration, in Marx. From an empirical, historical perspective, there is nothing wrong with this genealogical description, for it in fact reflects a well-known and widely accepted understanding of the development of Western philosophy. The problem is the rupture that enters the text at this juncture. For while the Radical Enlightenment tradition, with its emphasis on rigorous skepticism, scientistic and atheistic rationalism, emancipatory politics, and radical, revolutionary activism does indeed come to fruition in Marx, it does not end there so much as find a new catalyst for its on-going development. Israel, however, does not allow for any linkage whatsoever between the object of his history and philosophical radicalism today. He in fact effaces such a tie by making "western democratic libertarianism" and "emancipatory individualism" the most important legacies of the Radical Enlightenment while burying dozens of authentic twentieth-century heirs to the radical Spinozist philosophical tradition under the blanket label "postmodernism." Telling, in fact, is the way that for Israel the defeat of Marxism after 1945 is one of the noteworthy achievements of the ideas spawned by the Radical Enlightenment, while the role that this same radical

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philosophy played in making and sustaining Marxism in the first place is neither acknowledged nor referenced in the analysis.

In sum, the author of *Enlightenment Contested*, to use the book's own terminology, is ultimately a man of the Moderate Enlightenment, and accordingly his thoroughly un-radical history of radical philosophy works to appropriate certain features of it for middle of the road Enlightenment liberalism while dismissing many of its more philosophically provocative and politically radical legacies under the dismissive epithet "postmodernism." The result certainly works to give mainstream Western liberals an attractive revolutionary cachet. If one would rather read a less enthusiastic and European triumphalist account of the place of radical Spinozism in the making of European philosophical and political modernity then and now, far better to turn to the works of the radical philosopher and once imprisoned political exile and critic Antonio Negri. As a work of positivist, empirical history conceived to provide a meticulously accurate account of the intellectual figures and debates constitutive of the eighteenth-century making of philosophical modernity, this erudite and comprehensive work offers invaluable scholarly resources. Yet as an intervention into the historical understanding of this modernity, and as a work that seeks to wed history and philosophy to the benefit of our comprehension of both, *Enlightenment Contested* generates more frustration than satisfaction.

Nevertheless, the book and its prequel have largely been received with open arms by the global academy, and discussions of Israel and "his Enlightenment," like the seminar held in London in 2006, continue to proliferate. Israel's history of Enlightenment is also increasingly discussed and referenced everywhere from academic journals to contemporary news magazines. Part of the appeal is the present climate of opinion, which is exceedingly sympathetic to Israel's intellectual-political sensibility. The current vogue for celebrations of atheistic rationalism and the progressive politics that are alleged to follow from them provides, in fact, a welcoming environment for Israel's thick scholarly tomes. *Enlightenment Contested* could easily share a shelf labeled "Twenty-First Century Religious Wars: The Secularists" with Richard Dawkins's *The God Delusion* and Christopher Hitchens's *God Is Not Great: How Religion Poisons Everything*. Israel's erudite historical scholarship is certainly in a different genre entirely, and his work will no doubt survive long after these cries of the moment have been forgotten. Like La Vopa and other professors of intellectual history, I will also continue to modify my European intellectual history courses in accordance with the revisions compelled by Israel's impressive research. Nevertheless, once these revisions have been absorbed, I suspect that the wider phenomenon of "Israel's Enlightenment" will fade, and that his project will become, like those of Dawkins and Hitchens, an artifact of the peculiar vicissitudes of philosophical modernity and its politics in the first decade of the twenty-first century.

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Robert Service. *Comrades! A History of World Communism*. Cambridge, Mass.: Harvard University Press. 2007. Pp. xviii, 571. $35.00.

Some people say that if you know the dealings and doings of your own little patch, then you will also understand the world. As with most things people say, this is about half right: you will get some of it but miss out on quite a lot. This survey of the depressing history of world communism, written by one of the world's foremost experts on Russia, is a case in point (although the patch here is grander than most): it brilliantly captures the rise and fall of a Soviet-inspired ideology of the twentieth century, while leaving something to be desired in explaining why so many people around the world for a while flocked to support it. It is an admirable overview that could have been more solid on specifics outside Russia and Eastern Europe.

The book is superbly written and structured; Service is a master historian, as his biographies of V. I. Lenin and Joseph Stalin and his history of Russia show in full. This volume deals with an even bigger subject than his previous work. The first half of the book shows how Lenin's communism developed as a political theory and a movement on the left of European socialism, how it came to power in Russia and established the Soviet state, and how it became a world movement during and after World War II. The second and final half then discusses the challenges to Soviet control of the movement it had created and the resistance to communist dictatorships that emerged in the latter half of the twentieth century, up to the point when doubt, disintegration, and opposition together destroyed the project worldwide, including in the Soviet Union itself. Service presents us with a form of history that—admirably—deals not only with high politics but also with the experiences of communists (and anticommunists) who participated in the battles in which they believed that the future of the world would be decided. These clashes, as Service notes, took place as much inside trade unions and liberation fronts as in the confrontation between parties and states.

Overall, it is a dismal story. The amount of human folly that went into communist projects in different parts of the world is staggering, and the suffering and sacrifice (including self-sacrifice) that they demanded

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are astounding. Just think of the history of communism in Hungary from Béla Kun to János Kádár. In spite of being a small party in terms of its real support throughout these seventy years, with a following of at most one-fifth of the population at (for the communists) the best of times, it dragged the country through three major cataclysms: the Hungarian Soviet Republic of 1919 (which, as Service points out, collapsed in "blood and ignominy" after 133 days in power); the "transition to socialism" in 1944–1949 with its executions, prison camps, and deportations; and the 1956 revolution, crushed in blood on the streets, with 200,000 refugees as a result. For a small country in the center of Europe this is quite a tally. Coming on top of two world wars in which Hungary was on the losing side, it makes for a bad century.

Sometimes the folly was mainly folly, without massive criminal intent. The 1919 Bavarian Soviet Republic, headed by Max Levien and Eugen Leviné, did little except declare war on Switzerland and order all shops to close (the latter, presumably, played a not insignificant part in its rapid downfall). But in spite of its comic-strip character, the regime still helped pave the way for the crash of German democracy. Others were much worse. The Soviet-inspired Ethiopian regime of Mengistu Haile Mariam killed off a million and a half of its citizens during the 1970s and 1980s. The China-inspired Cambodian regime of Pol Pot and his comrades managed to murder almost two million.

Given this sordid and now well-documented record, there is little doubt that many communist leaders deserve all the scorn and condemnation that Service heaps upon them. But the condemnatory tone of the book seems sometimes to get in the way of explaining why so many people in different parts of the world chose communism as their life project. Service announces a more in-depth approach as part of his analysis from time to time, but he never really gets around to dealing with patterns of recruitment and support for communist parties worldwide. If he had done so, his survey would have been even more useful than it is in its current form.

What he would have found, I think, is this: people became communists for a wide variety of reasons, dependent on where they were from, on their personal experiences, and on their hopes for the future. In Eastern Europe after 1945, where communist rule was imposed by Soviet military power, many new recruits were driven by the search for power and status. In Western Europe, the United States, or Japan, joining the communist party was a certain road away from status and power. In parts of the Third World it was next to suicidal to join the communists; ending up in prison or before a firing squad was more likely than holding political office. In personal terms, the motivations were therefore as varied as the history and cultural backgrounds of the many places where communist parties were established.

Much of the power of communism in the latter kinds of countries came through the idealism and dedication shown by individual communists in fighting for causes that many people found important: racial equality, trade union rights, better working conditions, or increased wages. In some countries, such as those in southern Africa, communists stood for a relentless campaign against the white minority regimes (while far too many others were ready to make their peace with those in power in spite of the evil they represented). In Latin America, communists were often the only ones who stood for gradual change toward enfranchising the working class and full parliamentary democracy. (In Chile, for instance, they therefore found themselves on the right within Salvador Allende's coalition; to say that the coalition was "Communist-inspired" [p. 323] is to miss the point.) The key point is that communists did all of this—and often gave their lives for these causes—while continuing to defend the Soviet Union and the East European dictatorships, never understanding how morally, politically, and, eventually, economically bankrupt the regimes they supported were.

Another difficult issue, which Service is better on but never gets fully to grips with, is the enormous variety among communist parties with regard to how members interpreted their tasks within their own countries. While the book rightly points out that all communist parties were inspired by the Soviet Union, and that most had spent a substantial period under Moscow's direct tutelage (from the Comintern on), many came to see their domestic mission as very different from that seen by the leaders of the Soviet party. Mao Zedong's project for China, for instance, was from the beginning so "creative" that it hardly resembled Marxism at all (in spite of the Chinese leader's grasp of Stalin's methods). Still, it is important to understand that many of these variations came about not because of a conscious wish to break with Moscow, but simply because messages were misunderstood or misinterpreted, or adapted "creatively," or simply had to fit into a world of thought massively different from that of East Berlin or Moscow. While the study of communism needs more historians, it surely also needs anthropologists.

The troubled relationship between communism and modernity also needs a bit more attention than it gets in this survey. While Service is right in pointing out that communists tried, and failed, to present their project as the logical continuation of Western modernity, he mostly drops the curious connection between Marxism-Leninism and science that stood at the center of communist claims (and often gave them the time of day with people who for other reasons should have known better). The sense of knowing the future that communists presented was a powerful one, because it struck where Western liberalism is weakest: giving purpose and direction to those who see the ills of the world and want to remove them. Service's Oxford colleague, Sir Isaiah Berlin, wrote often about the ill-fated perception of a link between Marxism and science. It provided excuses for the most horrendous crimes, while steeling comrades for the trials and turbulence of preparing the workers for a revolution that was bound to come. As Berlin was fond of pointing out, it is when people most

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believe they know where they are going that they are likeliest to be wrong.

Service's discussion of the communists' relationship to their opponents is very good and useful. He makes the crucial point that in Europe those the communists hated most were often other left-wingers: social-democrats, socialists, and anarchists. Almost everywhere these groups hated the communists back with equal fervor. A significant part of the European civil wars of the twentieth century was therefore the struggle between Marxist-Leninists and their opponents on the left over control of trade unions, political fronts, and often over city blocks and streets. In a remarkable number of cases communists lost these contests; due to their own weakness, they were defeated and isolated on the left before they were turned upon by the right-wing establishment—except, of course, in those cases where they could depend on the Soviet army. The only exception is Italy, but then the Italian party achieved its hegemony on the left exactly because it tried, to Moscow's irritation, to be a broad church.

Outside Europe, however, the communists' opponents were not usually other leftists but rather nativists and religious groups. Some of these were of the more poisonous kind, and it would have been useful if Service had set off a bit more space to deal with their ascendance—greatly helped by the West—at the end of the Cold War. Service is right to say that in several places (notably the Balkans) communists showed that they could morph into extreme nationalists. But elsewhere the decline of the left often signaled the rise of the most unpalatable kind of right wing, especially, of course, in parts of the Muslim world.

Given the appeal of communism, why did the West win so decisively in the end, at least in Europe? Again, Service has in my view got it more or less right. It was when Western states became more inclusive, when—through state intervention—they began to offer health services and education for all, to protect workers' rights, and to give opportunities to women and minorities, that communism was doomed. Since it already had lost the battle over productivity, the appeal of the West was too strong to be resisted, as soon as an opportunity for change occurred. By the late 1980s the old Radio Yerevan jokes from thirty years earlier—Question: what is the average wage for an industrial worker in America? Answer: they shoot black people—had lost their edge; the scales of both productivity and justice had dipped decisively in favor of the West.

Service has written a very good overview of the rise and fall of communism from a Russian and European perspective. There is plenty of room for additions to this story, however, especially if we are to take Service's own injunctions at the end of the book seriously. "The self-interested domination of the world by a handful of great powers persists," he warns us. "There is much scope for radical movements to arise and challenge this situation." You bet. And some of them will certainly be inspired by communism as it grew and metamorphosed in the Third World.

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Charles Maier. *Among Empires: American Ascendancy and Its Predecessors*. Cambridge, Mass.: Harvard University Press. 2006. Pp. 373. $27.95.

Bernard Porter. *Empire and Superempire: Britain, America and the World*. New Haven, Conn.: Yale University Press. 2006. Pp. 211. $30.00.

That the United States was an empire appeared self-evident to some commentators on American history, particularly during crucial moments in the country's past. Henry Hugh Brackenridge, a Pennsylvania lawyer and later justice of the state's supreme court, extolled the young nation's imperial birth in the wake of the necessary nine states' ratification of the Constitution; whereas Frederick Jackson Turner, some fifty years later, looked at the western frontier and saw an expansion that was not unlike empires of the past. Nearly a century onward, Franklin D. Roosevelt saw America's global domination rising from the ashes of Britain's postwar ruin, with the imperial torch passed in an Anglo-American hand-off. Regardless of the era, the attitudes of American politicians and elites were highly ambivalent and complex with regard to empire, particularly the British Empire. *Pax Americana* was, for most, not *Pax Britannica*, particularly in the wake of the American Revolution. America would do what Britain could not, by peacefully expanding while also extending benign forces for progressive change. Yet, the inherent contradictions of American expansion were not lost on several observers, leaving Roosevelt later to express his genuine dislike of the hypocrisies and excesses of Britain's Empire while at the same time readying his nation to assume the Anglo-imperial mantle. At the heart of his ruminations were questions as to whether or not America was an empire, and if so, how it differed intrinsically from its predecessors, particularly the British?

Today such questions have taken on a sense of urgency. In the post–9/11 world, the spotlight has shifted to a presentist examination of a new global order. America's deployment of military might in Afghanistan

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and Iraq, the toppling of local regimes, the maintenance of occupational forces, and the incipient violence have combined to thrust empire into the center of scholarly and public debate.

Parallels between past and present abound, with many probing historical debate about Manifest Destiny or nineteenth-century India for its implications for caricature of twenty-first-century expansion and military intervention in Ramallah or Baghdad. Adding to this shift, as well as to a decidedly vituperative element of the debate, is the more ideological and intellectual dimension of empire's revival in the scholarly realm. If one were to draw lines, the so-called "new imperial historians" are on one side, inspired by the writings of Frantz Fanon and Edward Said, among other postcolonial theorists; on the other side are the "old imperial historians," whom the new imperial scholars often view as politically, intellectually, and methodologically conservative. The stark binary between "new" and "old" has given rise, at its worst, to a misleading set of questions. Was empire, by its ideological design, benign or exploitative? Is modernity and the current world order an epiphenomenon of imperial orders, past and present? Is the postcolonial world a product of, for example, British or French imperialism, or did empire play a minor role in the contemporary circumstances of nation-states like India or Vietnam? And, at its crudest level, were the empires of yesteryear, together with their contemporary successor, good or bad?

Empire is the most protean and pervasive political form. It is also one of the least understood. An integrated approach to its study demands, among other things, the scrutiny of political institutions and structures, movements of capital and the creation of markets, the roles of individuals at the highest and lowest levels of power across races and ethnicities, the place of ideology, and its impact upon the practice and perception of imperial rule at home and abroad, all within an analytical and methodological framework grounded in historical precedent and broad enough to read and interpret a variety of competing sources, some of which could be viewed as more reliable than others. In effect, I state what could be obvious to some: that we must begin our re-evaluation of empire by integrating the binary, which is, at once, both real and artificial, between the "new" and "old" imperial histories if we hold out any expectation of working toward a comprehensive understanding of one of history's most ubiquitous political forms.

Such an integrative approach is fraught with ideological, methodological, and practical challenges. To begin, how do we define and analyze empire when it has assumed a variety of forms across space and time and when the term itself is fraught with political meaning? Charles S. Maier sets out to confront the challenge of whether America can be defined as an empire, placing his analysis within a broadly comparative imperial framework that stretches back to antiquity. He examines the institutions and structures of American power in the nineteenth and twentieth centuries against the international and domestic features of previous empires before turning his analysis to the United States in the twenty-first century and its stated objectives in forging a new world order. Rather than committing to specific labels for contemporary American endeavors—he stops short of calling the United States an empire—Maier shows through historical comparison the qualities of empire shared by the United States and its predecessors. These include power, both hard and soft, as well as a willingness and capability of defining and defending frontiers of containment. In America's case, Maier locates hard power in the country's enormous military and economic capacities that were deployed in regions like Serbia and Iraq; soft power he sees in a popular cultural hegemony that transcends borders through prolific dissemination, particularly commercial and informational; and he demonstrates America's frontier containment through numerous examples, including Vietnam and Cold War Germany and, more recently, Kuwait.

The enormous value of Maier's work resides not only in his careful surveying of history for the definitional appropriateness of empire in the current American context, but also in his analysis of "what long-term implications for international and domestic society and politics arise if we in fact become an empire" (p. 12). It is in this light that he considers questions of whether empire translates into more equitable global economic development and welfare or greater polarization, and whether empire renders "world politics more peaceful or more violent" (p. 12). Within the limitations of his study, Maier offers exceptional insights into globalized mass consumption patterns as well as patterns of violence that have attenuated empires at particular moments in time, and that characterize imperial institutions more generally as they "tend to play on ethnic differences and polarize class differentiation, and thus remain prone to violence" (p. 129). While he remains cautious about a future American empire, as historically others "eroded individual liberties and marginalized dissent," he acknowledges that positive forces of cosmopolitanism and increased multiculturalism are potential benefits, together with the possibility of increased toleration, domestically, of immigrants and minorities (p. 294).

As Maier draws significant insight from a broad view of empire across time and space, Bernard Porter offers an exceedingly insightful vertical examination of the relationship between the British and American empires in his most recent undertaking, *Empire and Superempire: Britain, America, and the World*. While decidedly reluctant to "draw direct lessons from history"—that is, looking to "the old British Empire . . . to point useful 'lessons' for present-day America"—Porter offers a fascinating parallel study of the nineteenth-century British Empire and nineteenth and twentieth-century American imperialism, capped by a twenty-first-century American Empire (p. 5). Indeed, unlike Maier, Porter has no issue with the term empire to describe present-day American global dominance. He invokes a broad

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definition, reminiscent of Ronald Robinson and John Gallagher's imperialism of free trade, whereby empire is not restricted to the possession of colonies but rather extends to the exercise of one nation's economic and political domination over that of another. At issue for Porter is what he perceives to be a significant misreading of the British Empire that, in turn, has given rise to a misreading of American imperialism; he seeks to set the record straight about nineteenth-century British imperialism and, with this comparative corrective, illuminate American imperial enterprises.

For Porter, the similarities between British and American imperialism are undeniable, in fact, the two are downright "uncannily close" (p. 7). He reads this in a variety of realms, starting with imperial denial. Like America of today, Britain of the nineteenth century also refused to acknowledge its imperial pursuits. "It is easily possible to imagine Gladstone," Porter writes, "saying, 'we don't do empire,' even while he was in the process of invading Egypt" (pp. 3–4). Moreover, he provides further evidence that the United States was an imperial nation from the start, stating "the idea of America as an 'anti-imperialist' power, in fact, is a fairly recent construction"; or, put another way, American imperialism is not a present-day, neo-conservative creation (p. 64). Drawing on his previous works, Porter then suggests that Britain's history and culture were far less imperialistic, and certainly some individuals more ambivalent (e.g., John Stuart Mill and Adam Smith) about the imperial project, than most Americans have imagined. Parallels continue, with capitalist expansion, or Britain's free trade and America's globalization, another point of intersection. It is in the realm of ideological claims, however, that Porter sees a major point of departure, with America's Christian Right, in particular, shifting a shared Anglo-American civilizing claim to a squarely American-style conquest predicated on liberation. This, when combined with a military, economic, and cultural hegemony, exceeded any previous empires, thus transforming America into what Porter deems a "superempire."

As we move toward a more integrated study of empire, the works of Maier and Porter have much to offer. They present systematic attempts at defining and analyzing empire comparatively and as such illuminate particular qualities of the political form, and the ideologies accompanying it, that transcend time and space. They also, through their strengths and limitations, point us to areas of additional inquiry and synthesis, areas which present their own unique challenges. In the first instance, I would like to turn my attention to the question of turning away: that is, to the turning away from empire that characterized many Britons in the past as well as Americans in the present, a turning away that, to varying degrees, has influenced imperial as well as more recent productions of knowledge. Reflecting on this phenomenon, Linda Colley suggests that "Since empire, in Joseph Conrad's words, is frequently 'not a pretty thing when you look into it too much,' some Britons in the past (arguably again like some Americans now) may have preferred not to examine their polity's global interventions too closely at all" ("The Difficulties of Empire: Present, Past, and Future," *Historical Research* 79: 205 [2006]: 376).

The issue of confronting a Conradesque horror is hardly a condition unique to imperial historiography. Indeed, the difficulties encountered when confronting unsettling or violent acts—to bear witness as either a historical actor or a historian—are something that most individuals would prefer to avoid. When the specter of violence is observed, it can often be difficult to describe. Yet, the examination of violence has become acutely relevant in contemporary times and particularly to the study of empire, as it is, in part, the very nature of the current war on terror and America's global expansion that has fueled empire's fashionability as an area of scholarly inquiry. But the significance of interrogating violence goes beyond the current events fueling a rethinking of empire. Without confronting and analyzing violence, whether in empire or elsewhere in history, we can not explain its nuances, its varying degrees, or its connections to broader institutions and processes. Nor can we begin to understand how people have undertaken barbarous actions if we do not know precisely what those actions were. Indeed, in the study of the British Empire, we know very little at all about the ordinary people, whether they be members of the indigenous populations or poor whites who comprised a large segment of the European population, particularly in the realm of police enforcement.

The current divide between "new" and "old" imperial histories can be located partly in this question of colonial violence, or more broadly in what Nicholas Dirks has recently termed scandal (*The Scandal of Empire: India and the Creation of Imperial Britain* [2006]). For some, there is a corrective underway, but there is also the danger of carrying this corrective too far, such that we are faced with a polarized set of texts that present the British Empire as either uniformly exploitative or as relatively benign, albeit with a few exceptions acknowledged. In my own work on Kenya's detention camps in the 1950s, I was interested in documenting and witnessing the violence largely because no one had yet done so. Certainly we historians of Kenya knew of the camps and the violence, just as contemporary actors in Kenya and Britain knew, but in some fundamental ways we were like those who turned away in the 1950s. We, too, did not know. Our historical questioning led us away from confronting the horror, and this, in turn, created a lacuna of knowledge that frustrated our understanding not just of Kenya, but of broader postwar imperial processes. Yet, the establishment of empirical evidence should not be conflated with sweeping interpretations of Britain's Empire, thus producing the historiographical dilemma of "new imperial history" versus "old-style imperial history," or even worse "good" versus "bad." Instead, we in the field are faced with the challenge of working assiduously toward integrating new empirical findings—whether on, among other topics, violence or the role of the marginalized—that seek

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to generate fresh understandings based upon the introduction of this evidence, rather than on its rejection, either in part or in whole. And, as Maier and Porter have usefully demonstrated, empire is also about the movement and connection of ideas, as well as people, between the metropole and periphery, or frontier, and within and between the frontiers themselves. We are just beginning to understand these processes, in part because of new historical evidence, and in part because of new frameworks that accommodate imperial diasporas of ideas and people.

This brings me to my final point regarding the methodological and practical challenges we confront. In their recently published *Forgotten Wars: Freedom and Revolution in Southeast* Asia (2007), Christopher Bayly and Timothy Harper have produced precisely the kind of integrative approach connecting the movements of ideas and individuals that the study of empire demands. Yet their narrative and analysis on the Malayan Emergency present a challenge, even in this innovative work. Bayly and Harper concede that the official records relating to Batang Kali, like most files related to law and order during the emergency, were destroyed under the terms of Section 6 of the Public Records Act "'as not being worthy of permanent preservation'" (p. 453). They then go on to write that "hundreds of thousands of people would be ensnared by this crisis [of terror and counter-terror]. This too has dropped from historical memory; a forgotten story of a forgotten war" (p. 456).

But has it dropped from historical memory or merely from official memory? When examining our methodological concerns, and our need to access sources that push against the grain of imperial knowledge, we should carefully consider A. J. Stockwell's recent invocations regarding the official colonial records. "Of the files that survive," he writes, "many may obfuscate the aims and motives of decision-makers or acquire in retrospect a significance which they did not enjoy at the time of writing. Yet none of this nullifies their value provided they are interrogated, like any other source, with skepticism and imagination as well as with knowledge and understanding of their historical context" ("British Decolonization: The Record and the Records," *Contemporary European History* 15: 4 [2006]: 583). But, I would urge us to reflect upon whether it is possible to imagine the unimaginable. Moreover, is imaginative textual interrogation only possible when employing other methodological alternatives, particularly those used by some historians also trained in area studies? Oral history is an important category of historical evidence, and without it, historical memory, as Bayly and Harper point out, is in danger of being erased, and the story "forgotten." Of course, like other texts, oral histories must be interrogated, but they must not be dismissed. To do so is to run the danger of reinforcing enduring patterns of paternalism and imperial knowledge, and, in some cases, impugning out of hand the individual or individuals who have born witness and who have the most to say about a particular experience. We should never ignore those who did not turn away. Indeed, on a practical level the integration of a multi-archival, multinational source base, together with a collection of oral testimonies that accounts for variables including gender, generation, and geography, is a Herculean task. Nevertheless, as we seek a more comprehensive approach to empire, and as the scholarly and public communities search our work for historical lessons, we must build upon the innovative work of our professional colleagues, and bring with it a methodological rigor that challenges imperial knowledge in new ways.

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JOAN SHELLEY RUBIN. *Songs of Ourselves: The Uses of Poetry in America*. Cambridge, Mass.: Belknap Press of Harvard University Press. 2007. Pp. 470. $29.95.

Joan Shelley Rubin's book announces its thesis with its title: it argues that poetry was not just enjoyable, but also somehow *useful*, to twentieth-century American readers. Like Rubin's first book, *The Making of Middlebrow Culture* (1992), this one is a readable synthesis, drawing primary and secondary sources together to forge a satisfyingly concrete narrative. The "history of the book" is a growth field among literary scholars, and Rubin uses its premises as her guide, assuming that readers make texts meaningful and that reading is a social act. Rubin organizes her discussion around representative historical anecdotes, but she opens her book with a more personal story: as her father lay dying, she read aloud to him from a poetry anthology, *Through Magic Casements* (1927), that her father had loved in his youth. As she read, she realized that "the poems had taken on a function unrelated to their subject and structure: they had given me a way to cope with my distress at not knowing, in those pained circumstances, what to do" (p. 2). Exploring the function of popular poems thus becomes the aim of Rubin's study.

The book is divided into two sections. Part one describes how poetry was produced and marketed between 1880 and 1950, concentrating on how and why it was (or was not) popular. Part two turns to specific sites—schoolrooms, households, churches, summer camps—in which poetry flourished as a social art. This bipartite structure works well, because it reflects the different ways that poetry publishers and poetry readers used their materials. Publishing history in the 1920s, for instance, has little to do with Henry Wadsworth Longfellow—and yet readers in the 1920s continued to recite

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him. One of this book's many strengths, then, is the way its lucid organizational structure contributes to its critique of "standard" literary periodization by producing two parallel accounts.

Each chapter in the first section revolves around an opposition or tension. "Seer and Sage," for instance, identifies a paradox at the heart of the Emersonian "model poet," who must be at once an esoteric individual and a democratic community builder. Rubin echoes Christoph Irmscher's recent insights about public poetry (*Longfellow Redux* [2006]) while making a broader point. American popular poets, she argues, could not and did not separate themselves from their audiences. In "Amateur and Professional," Rubin extends this idea, exploring how nineteenth and early twentieth-century poets walked a thin line: on the one hand, they sought commercial visibility, unlike their high modernist successors; but on the other hand, they invited readers to identify closely with them. Readers responded to poetry by penning their own verses, sometimes in direct homage to their beloved authors who served as both "icons and friends" (p. 33).

Revising the old story of modernism's march, in which Ezra Pound rescues readers from the inane clutches of James Whitcomb Riley, Rubin describes American poetry as a changing field of cultural negotiation. Older poems coexisted with new work, and modernity reached the masses in fits and starts. The figure of Edna St. Vincent Millay, for example, was both *outré* and popular; marketed as a modernist, she wrote in largely traditional forms and managed to be a crossover hit adored in both Greenwich Village and Greenwich, Connecticut. High modernists like Pound are peripheral to the action, since in Rubin's revisionist view their disdain for audiences rendered them unusable to most readers. This focus on widely circulated poets mitigates—without overturning—the theory that poetry reading declined in the twentieth century. Perhaps the most useful aspect of the book's first section is its careful attention to the facts and figures of publishing history. Why didn't poetry sell? Or did it? Why were some poets popular, but not others? Rubin does not tackle these questions speculatively or even very analytically; instead, she provides documentary evidence (sales numbers, for instance) that shows how publishers and the public struggled to "place" poetry in the context of twentieth-century American life.

Having addressed publishing history, the book's second half turns to poetry's readers. In this section, the anecdotes become even livelier as Rubin evokes—through diaries, letters, fictional scenes, and first-hand accounts—the ways that people made poetry meaningful. Her overarching thesis throughout is that simply reading the texts of poems does not reveal their meanings; rather, to understand what poems mean, one must attend to the dialectical relationship between individuals and their social worlds. The first world that Rubin explores, in "Listen My Children," is the schoolroom. In *Schoolroom Poets* (2005), I argued that nineteenth-century people first learned to recite poetry in school, and thus for many adults the meanings of "best-loved" poems were bound up with their memories of childhood. Rubin extends the schoolroom tradition through the interwar period and into the 1950s, exploring the choric speech craze, the impact of Sigmund Freud on theories of child development, and the trickle-down effect of the New Criticism on literary pedagogy. She shows that even after the "schoolroom poets" died, their cultural work continued and evolved.

The next chapter, "I Am an American," traces the closely related practice of assimilating immigrants through pageants, settlement house activities, wartime propaganda efforts, and citizenship handbooks that linked poetry reading to civic engagement. Two central points surface in these chapters: first, that all reading (even silent reading) is socially conditioned, albeit not fully determined; and second, that poetry-reading patterns among ordinary people do not necessarily reflect the divisions (nineteenth vs. twentieth century, lowbrow vs. highbrow vs. middlebrow) that critics have imposed on literary history. This is a useful insight, not just for literary historians in general, but also in light of Rubin's earlier work, with its emphasis on cultural hierarchies.

While many historians and critics have explored the relationship between modernism and domesticity—see, for instance, Suzanne Clark's *Sentimental Modernism: Women Writers and the Revolution of the Word* (1991)—the nexus of modernism, domesticity, and popular poetry has been largely overlooked. As Rubin shows, the home was a key site, not just for poetry reading but also for negotiating and moderating, *through poetry*, the shock of the new. Moreover, poetry took on extraliterary duties in the domestic sphere, serving to cement affective bonds between parents, children, friends, and lovers. Such affective bonds even extended to poets themselves, as Rubin shows in an extended discussion of Carl Sandburg's fan mail. As a Texas woman put it in the late 1920s, "You have spoken to me individually in the loneliness of my heart and I have found in your sympathy, [the] understanding and encouragement to go on telling the story of my kind of people" (p. 284).

Rubin's examination of religious poetry readers, in "God's in His Heaven," further illustrates her point that a poem's meaning is social and contextual. For instance, the Baptist missionary Isabel Crawford kept voluminous scrapbook/diaries, compiling verses by Millay, Edgar Guest, and Longfellow and "giving them new meaning as sources for the renewal of her Christian commitments" (p. 306). Moreover, like other religious diarists, Crawford did not reject literary modernity but rather integrated it into her genteel, Protestant worldview. An even more striking example of religious appropriation emerges in Rubin's discussion of "poetry as prayer in Reform Judaism" (p. 329). In the mid-to-late twentieth century, Reform prayer books incorporated, and sacralized, secular poems by authors such as Stephen Spender and Anthony Hecht. The meanings of these poets' words were thus utterly transformed by

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their new institutional context as part of a worship service.

Rubin avoids quoting extensively from poems or analyzing their contents, partly to underline her point that meanings are not generated by texts alone, and partly because she is deliberately writing cultural, not literary, history. Unlike many academic writers, she understands that one cannot focus on everything. However, by the end of the book poetry lovers might begin to feel that they are listening to an art history lecture without the slides. It is thus a joy when Rubin opens her final full chapter, on outdoor poetry, with a brief and brilliant analysis of Joyce Kilmer's "Trees." She argues that the poem poses a central question regarding poetry's use-value; to wit, what is the relationship between poetry reading and lived experience? Through descriptions of John Burroughs, cowboy poetry, and formal camping activities, Rubin traces the tension between outdoor poetry and nature, reflecting the anxieties about over-civilization described by Jackson Lears in *No Place of Grace: Antimodernism in the Transformation of American Culture* (1981). Poetry became both a way to appreciate the outdoors and a potential impediment to real life. Here Rubin hits upon one of poetry's central limitations as a cultural bellwether: even "popular" poetry appeals to a certain subset of people—the bookish types who recite around the campfire while everyone else goes swimming. Given the anti-intellectual strains in American culture, even mainstream poetry is, in some sense, marginal.

"It would be easy," Rubin concedes in a coda, "to see the history of poetry reading in America as a story of decline and fall" (p. 381). Instead, she offers a more mixed assessment, based on the responses to Robert Pinsky's 1997 "favorite poems project," in which he asked Americans to explain why they loved a particular poem. Strikingly, people's accounts involved not just interpreting poems but also remaking their meanings; a student nurse, for instance, saw her patients' pain reflected in Edgar Allan Poe's "Dream within a Dream" (pp. 402–403). Even today, then, poems work as "songs of ourselves" and repositories of our memories and our hopes. Or at least, they work that way outside of college classroom settings, which should raise serious pedagogical questions for those of us who teach poetry. Are we helping our students to love poetry, and if we are not, should we be? Is the main cultural work of poetry more affective than intellectual?

This book deserves a wide audience, both within academia and outside of it. It is a scrupulously professional piece of scholarship, but it is also animated by Rubin's personal enthusiasm for poetry. Like many books that straddle the academic/popular fence, it does present some minor frustrations for scholars. There is no bibliography, and while Rubin's avoidance of academic hairsplitting makes her book more readable, it can also make it hard to trace whose ideas are being used or refuted at any given point. Moreover, cultural critics are privileged over literary historians, so that some recent work goes unused or underused. Thus Rubin identifies the "girl poet" as a phenomenon of the 1920s, but in fact this is an old pattern in American literary history, dating back to at least the 1820s, as Mary Loeffelholz points out in *From Schoolroom to Salon: Reading Nineteenth-Century American Women's Poetry* (2004). Rubin footnotes Loeffelholz but does not take advantage of her insights. Other books, such as Mary Louise Kete's *Sentimental Collaborations: Mourning and Middle-Class Identity in Nineteenth-Century America* (2000), are entirely overlooked, even though Kete's discussion of poetry scrapbooks and affective bonds would surely contribute to the historical depth of Rubin's analysis. However, my complaints are themselves in the tradition of academic hairsplitting, and Rubin writes fluently precisely because she chooses to streamline her story.

Ultimately, this is a thrilling, sweeping overview of how—and why—poetry mattered (and still matters) to its readers. At the same time, it moves far beyond the "history of the book" as it grapples with the shifting relationships among individuals, texts, and institutions. Moreover, in our era of so-called culture wars, it is refreshing to be reminded that most people read for pleasure, not ideology. Early twentieth-century modernists fought anti-modernists in the pages of *Poetry* magazine, but ordinary readers blithely crossed the battle lines to read what they loved.

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Reviews of Books

METHODS/THEORY

CHRISTIAN JACOB. *The Sovereign Map: Theoretical Approaches in Cartography throughout History*. Translated by TOM CONLEY. Edited by EDWARD H. DAHL. Chicago: University of Chicago Press. 2006. Pp. xxiii, 417. $60.00.

Christian Jacob's stated ambition in writing *L'empire des cartes: Approche théorique de la cartographie à travers l'histoire* (1992) was to provide a prolegomenon to the study of the history of cartography from an informed theoretical perspective. Shaped by a variety of intellectual influences—most obviously Michel de Certeau, J. B. Harley, Jacques Derrida, Michel Foucault, and Roland Barthes (*L'empire des signes* of the last named obviously providing the inspiration for the title of the work)—the book was an ambitious recasting of long-standing problems in the study of maps and an identification of many more. Tom Conley's elegant English translation should introduce Jacob's provocative theoretical perspective to a wider audience of anglophone historians, cartographers, and literary theorists.

The volume is divided into four substantial essays, each of which more or less stands alone, although shared thematic concerns bind many of them together. Central to all is the rejection of a "positivist" approach to the history of cartography, which adopts a more or less teleological narrative of the steady accumulation of knowledge about the physical world, and the concomitant development of new media for expressing these findings. Instead, maps are regarded throughout as constructed texts, whose meanings are determined as much by the peculiar interaction of text and reader as by their representation of an unchanging geographical referent. This is not to suggest that the volume ignores historical development in the process of mapping: Jacob's discussion of the importance of the development of the printing press to the reproduction of maps is an exemplary treatment of a complex issue. Likewise, at several points he alludes significantly to the changes that satellite mapping have wrought upon the ways in which modern societies conceptualize the world. This latter point might have been developed more fully, particularly in this expanded edition (one wonders, for example, what Jacob makes of Google Earth), but an obvious grasp of the importance of technological evolution pervades the study.

Yet Jacob's primary interest is not the map as a mimesis of the world but as a reflection of the society that produced it. Early in his first essay, a central contention is made clear: that a map is only as good as the authority that it can claim. Regardless of its "accuracy" from a strictly empiricist perspective, a map commonly regarded as trustworthy retains its value, and one that has been discredited is all but useless. As a result, the history of cartography may be read not simply as the steady accretion of geographical knowledge and its transmission to an eager and open-minded public, but rather as an intense (and never-ending) struggle for the invaluable imprimatur of trustworthiness. Crucially, the "testing" of the map is itself a problematic exercise. The referent of a map of a large area is effectively invisible to the user on the ground, and created only by his or her accumulated geographical knowledge, and by the map itself. Authority, then, must come from somewhere else. Jacob's penetrating study highlights the importance of decorative convention, textual addenda, and cartographic glyphs to the creation of this trust.

A second major theme developed over the course of the book is the response of the individual viewer to the map itself. Jacob discusses both the position of the viewer outside the map (an image of the world inscribed in the floor places the viewer in a dramatically different position from a display map hanging on the wall, or a navigational chart held in the hands) and the imaginative processes by which the "reading" of such texts translate the imagination of the viewer onto the mapped surface. As the viewer's gaze wanders over the map, in part drawn by the intentional design of the cartographer, in part creating new itineraries from the unexpected juxtapositions of represented places, he or she occupies the represented space and takes possession of it in a unique way. Maps, then, are tools of power: a familiar enough Foucauldian contention, but one taken in unexpected directions here.

The book is a rich resource of observations and contentions of the kind, but it creates problems even as it solves others. In the long opening chapter, Jacob confronts the very definition of the "map." The section highlights just how elusive the concept is. A map can be produced on paper, parchment or papyrus, skin, stone, sand, or (now) the computer screen. It might seek to recreate the physical world at a variety of different

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scales or the fictional world of the novel. It might be cosmological or allegorical (categories that would include those peculiar "route maps to temperance" so popular in the late nineteenth and early twentieth centuries as well as the great *mappae mundi* of the medieval West). While Jacob's conclusions about the precise connotations of the term are provocatively open-ended, for much of the volume he returns to a number of paradigmatic examples, which subscribe more or less directly to what most readers would consider to be a map: the representation of the Indian Ocean in the Miller atlas of ca. 1519; Fra Mauro's *mappa mundi* of 1459; and even Robert Louis Stevenson's map of *Treasure Island* become models for understanding both verisimilitude and the necessary relation between single narrative and the multiplicity of possible narratives that the map opens up.

By his own confession, Jacob's frame of reference for the work is primarily "western." His previous work in the study of ancient geography is complemented by a diverse range of examples. The absence of a more thorough reference to extra-European mapping is perhaps limiting, but the range of this volume remains extraordinary: from Mercator, Miller, and the work of the μ group to *Earthsea* and the world of *Swallows and Amazons*. This gives Jacob an extraordinarily generous space for exploration. The result is a remarkable piece of scholarship.

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Gary Jensen. *The Path of the Devil: Early Modern Witch Hunts*. Lanham, Md.: Rowman and Littlefield. 2007. Pp. x, 283. Cloth $75.00, paper $26.95.

By applying various sociological methodologies to the early modern witch hunts, Gary Jensen evaluates the most prominent explanatory theories. As a book written by a sociologist for sociologists, it is tough reading for historians not intimately acquainted with statistical analysis. Unexplained jargon and statistical formulae make it difficult to evaluate detailed conclusions drawn from "bivariate correlations," tree ring studies, "ARIMA adjustments," probability rates, and so on. Moreover, despite quoting Lawrence Stone's caution about the dangers of converting "fuzzy" and incomplete early modern data into "hard statistics and graphs" that give false impressions of scientific accuracy, Jensen proceeds with the exercise anyway. That said, his data analysis is at least as rigorous as that of those who have conjured up biological or other single cause explanations of witch panics. Anyone tempted by such simplistic theories should read this book.

Opening with a neat distillation of various approaches to the witch hunts, Jensen then empirically tests these as much as the data will allow. Most fall into the category of "apocalyptic variables": epidemiological crises (ergot poisoning, syphilis, sleeping sickness, bubonic plague), warfare, climate change, or famine. In chapter six Jensen applies psychological and sociological theories of scapegoating, collective conflict, and "victimology" to the question of the "selection of targets." He discredits all single-source theories and supports the multicausal conclusions of the major historians in the field. For example, he supports H. C. Erik Midelfort's observation that war acted as a brake on witch hunting, Brian Levack's conclusion that the presence of strong central courts and governments performed a similar service, and Wolfgang Behringer's correlation of worsening weather or epidemic disease with the rise in witch panics. And, if a region already had real heretics or other threats to deal with, it did not turn to imaginary ones. Jensen appropriately critiques theories of a "women's holocaust" or an attack on female healers as explanations of why women were the principal victims. Instead, he supports Robin Brigg's point that local communal conflict and long simmering suspicion of troublesome neighbors lay behind the initial accusations. Elites needed, Jensen affirms, to accept the demonological stereotype of a conspiracy of sectarian witches in order for an accusation of witchcraft to progress beyond the initial suspects. In sociological terminology, Jensen explains, "scapegoating becomes particularly intense when it is encouraged or facilitated by elite constructions or cultural templates defining the hidden cause of collective misfortune, and when gatekeepers are receptive to complaints that can be fit into that template" (p. 172).

On the switch from real to imaginary enemies or scapegoats, Jensen's normally careful sifting of evidence falters, for he does not address the fusion of real Waldensian heretics with imaginary diabolical witches that by the 1420s had established the notion of a sectarian, diabolical conspiracy of witches. Instead, after noting linkages between plague outbreaks and pogroms against Jews, he focuses on the persecution of Beguines as setting the stage for a deepened suspicion of women. Yet, this latter persecution was essentially a fourteenth-century urban enterprise only obliquely related to rural witch accusations. Nor does Jensen deal significantly with the Reformation era's demonization and persecution of other heretics, notably Anabaptists, which in many regions immediately preceded the revival of witch hunting. Even so, when he turns to the Salem witch panic of 1692, he notes that the official ending of discrimination toward Quakers made a search for alternate scapegoats more likely.

In chapter seven, Jensen discusses for the first time the motivation and behavior of individuals. Until this point, readers might assume that, with the right confluence of apocalyptic factors, a populace will automatically seek out scapegoats and latch onto imaginary ones if real ones are absent. Jensen shows that one need not posit biological or psychopathological explanations for the behavior of the young women who began this panic. They knew how demoniacs were expected to behave, they were inspired by the religious and demonological beliefs of their elders, and they had good reason not to retract their accusations once the trials had be-

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gun. The level of credulity of individual preachers was an important but ultimately unpredictable factor.

Jensen's final chapter on modern witch hunts includes both "real" ones in India and metaphorical ones in the United States such as the anticommunist hunts in the 1940s and 1950s and the recent ritual child abuse "moral panics." He explores the applicability of his witch hunt criteria for these modern cases, and finds a pretty good fit, although he wisely cautions that it is too early to make anything more than suggestions. And, while scientific approaches to disease control have weakened the propensity to identify scapegoats, they have not slain that beast, especially with fundamentalist preachers fulminating about divine retribution for national sins.

A number of typos mar the text, including a clunker on page 137 that has the "Royalists," rather than the "roundheads," defeating the king's army during the English Civil War. Otherwise, Jensen's book is a necessary corrective for overly facile explanations of one of history's most puzzling episodes.

GARY K. WAITE
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NICHOLAS ONUF and PETER ONUF. *Nations, Markets, and War: Modern History and the American Civil War*. Charlottesville: University of Virginia Press. 2006. Pp. xii, 362. $45.00.

Ideas matter in history. They also matter in politics. How much and in what way they mattered in the making of the American Civil War is the problem posed by this book. Describing their method as a "systematic marshaling of evidence in support of thick description" (p. 18), Nicholas Onuf and Peter Onuf devote most of their book to the emergence and evolving meaning of concepts such as "nation," "markets," and "civil society" in the works of Thomas Hobbes, Jean-Jacques Rousseau, John Locke, Samuel Pufendorf, and, most importantly, Adam Smith. They then interpret the coming of the American Civil War as a consequence of modern understandings of the preconditions for a viable nation-state. As they emphasize repeatedly, these preconditions applied to all nations in the West during the nineteenth century but an understanding of those requirements was most advanced in the antebellum United States.

Briefly sketched, the Onufs' thesis convincingly posits an interdependent and thus co-constitutive construction of liberal notions of the individual, society, markets, and nations. At the heart of this ensemble of mutually defined concepts lies a logical conundrum in that the full realization of the benefits of a division of labor requires the extension of unrestricted markets to the entire world and, thus, the destruction of national boundaries, at least in the form of limits to markets. However, if nations are unequally developed in terms of technological capacity and capital endowments, then the most advanced of these nations will prey upon the less advanced in a global free market. For that reason, less advanced nations must protect themselves, usually through tariffs on imported goods from the more advanced nations, until they themselves reach equality with those more advanced nations. Thus, the path to a global division of labor (and an implied weakening or even elimination of the nation-state) somewhat paradoxically must traverse a more or less lengthy period of partial withdrawal from the world economy in which the nation-state is strengthened.

Furthermore, since the only way that weaker nations may defend themselves from the predation of stronger nations is through force of arms, tariff protection is also necessary for providing the industrial base for creating and maintaining a modern military. In the antebellum United States, the North maintained that war with Great Britain or some other European power was more or less likely and, for that and other reasons, tariff protection for manufactured goods was vitally necessary. The South contended both that war was unlikely and that the comparative advantage of the United States in the production of agricultural commodities (mainly cotton) meant that the nation could embrace global free trade with impunity. This conflict over national destiny, founded in the first instance upon trade and the prospects for war, was a major contributor to the creation of separate national identities in the North and in the South. The authors also acknowledge slavery as a proximate cause of sectional tensions, but they see southern and northern society as equally liberal within their meaning of the term. And this appears to make sectional conflict over slavery a more or less epiphenomenal byproduct of the deeper incompatibility arising out of the emergence of separate national identities.

That ideas matter in history and politics is beyond dispute. How they matter is debatable. Ideas undeniably construct the actors and institutions that animate social and political life. So, when the authors trace the construction of the idea of the "nation" and then put that construct in play as the object of politics, they are surely on firm ground. And they are also on defensible terrain when they present extensive evidence of this construction in the pamphlets published in the last decade before the cannons fired at Fort Sumter. However, much of the book takes the form of a very carefully executed treatise on political philosophy, a treatise in which we are warmly invited to entertain novel (and very seductive) readings of some of the most influential texts in political thought. The sophistication of this treatise goes far beyond what the treatise writers just before the Civil War ever exhibited and offers us at least some interpretations that those writers never entertained. Although they make a very interesting (and, to this reader, persuasive) case that Abraham Lincoln's political writing was compatible with their thesis, we are not given much evidence that he was as much an heir to the ancients as he was an innovating thinker in his own right. While that does not mean that seventeenth and eighteenth-century philosophers did not, in some sense, shape the modern world in which Lincoln brought forth his own concepts, it does mean that we do

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not know how those philosophers might have influenced how Lincoln actually thought his way through the problems of statecraft. And that means that the exquisite care with which the authors have traced the co-constitutive emergence of the building blocks of modern political economy has a more or less ambiguous connection to the coming of the "first modern war."

RICHARD BENSEL
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DONALD R. KELLEY. *Frontiers of History: Historical Inquiry in the Twentieth Century*. New Haven, Conn.: Yale University Press. 2006. Pp. ix, 298. $45.00.

Donald R. Kelley's book completes his trilogy on the history of historical writing. *Faces of History: Historical Inquiry from Herodotus to Herder* (1999) examined the development of historical studies from Herodotus and Thucydides to the European Enlightenment. The second volume of this project, *Fortunes of History: Historical Inquiry from Herder to Huizinga* (2003), traced the development of the genres of history in the glory years of historicism from Herder through the first part of the twentieth century. Kelley, an extraordinarily gifted historian of the Renaissance and long-term editor of the *Journal of the History of Ideas* (1985–2005), has a passion for the variety of historiography. In this latest volume his passion is tested by the extreme fragmentation of historical discourses. Others might have looked for a single cutting edge to unify this disparate field, but he has (wisely) found a variety of frontiers.

Kelley began with two "faces" of history. Herodotus represents a delight in the study of the local, or the consideration of society in its everyday forms. For Kelley, Thucydides represents a hunger for understanding the political dimensions of experience, especially the political decisions affecting large numbers of people. Kelley sometimes adds a third, Livian, filter representing a geographical or cultural focus on group identity. But he is most concerned with how the tension between the social and the political, between the anthropological and the institutional, can animate diverse historiographical traditions. His focus is on German, French, British, and American historiography. He recognizes that this is a Eurocentric survey, but the reader is grateful for the breadth of his interests and the depth of his learning.

The book does not maintain a clear chronology. Kelley is often pulled out of the time period that is supposed to be the focus of a particular chapter because he wants to follow a thematic thread. In the end, this makes for some ambiguity. For example, one would be hard pressed to say, on the basis of this book, how World War I affected the way German history was written in Paris. Nor is it clear how the civil rights movement affected the writing of nineteenth-century American history after World War II. Kelley nods to historical context, but given the wide-ranging nature of his narrative, he cannot thicken his descriptions of how a school of historical writing was formed. There is so much writing to consider, as he notes, that one is often reduced to producing an annotated bibliography.

If Kelley has little time for developing the context around his authors, he has even less interest in their theoretical presuppositions. One will not find a taxonomy of historical writing here. Indeed, one of the only extended discussions of a historian's work is a critique of Hayden White's *Metahistory: The Historical Imagination in Nineteenth Century Europe* (1973). White's seminal formalism is anathema to Kelley, perhaps because it displaces historical *practice* from center stage. A theoretical focus on historical *writing* seems to Kelley to be an abandonment of the "heuristic research" that makes that writing possible in the first place.

Kelley's own position is self-consciously hermeneutic, and he seems at home with Paul Ricoeur and Hans-Georg Gadamer. "For me hermeneutics means ... what Nietzsche called the 'interpretative character of everything that happens'" (p. 214). This does not lead him to radical skepticism or relativism, but to a modest proposal to think of history as "a constructed meaning at a present cultural moment and in a local cultural context" (p. 214). He turns to history not for *the* story, but for *a* story that can be told in relation to others. He wants history to widen the circle of narratives with which we are acquainted, and that we use to make sense of ourselves.

Aware of the hermeneutic condition of meaning making, Kelley is dedicated to understanding how others have made sense of this condition before him. The complaint that surfaces throughout the book is that other writers do not realize that what they are saying has been said before. Historians often think they are making progress or introducing new ideas, when they are only reinventing the wheel. Sometimes it seems that Kelley has read so much history that any idea that he comes upon will already appear old. Seeing only repetition was one of the dangers of historical study that Friedrich Nietzsche worried about in *The Use and Abuse of History*. Indeed, Kelley inadvertently enacts this danger in repeating almost an entire paragraph verbatim on pages 194 and 234.

This book does, however, go beyond saying that the more things change the more they stay the same. At his best, Kelley exemplifies a productive piety for the tradition of history writing: "One of the first motives of historical study was the honoring and the memorializing of ancestors and their predicaments, and in professional terms it is essential at least not to forget the two and half millennia tradition on which our practice rests" (p. 195). Kelley's act of memorializing is a mighty contribution to our disciplinary self-consciousness—a very honorable contribution, indeed.

MICHAEL S. ROTH
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MICHAEL E. HARKIN and DAVID RICH LEWIS, editors. *Native Americans and the Environment: Perspectives on the Ecological Indian*. Lincoln: University of Nebraska Press. 2007. Pp. xxxiv, 367. $24.95.

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In 2002, a group of scholars gathered at the University of Wyoming's American Heritage Center for an interdisciplinary symposium. Michael E. Harkin and David Rich Lewis have edited the perspectives of that group for this volume, which considers topics from Pleistocene specie extinctions to nuclear waste storage, focusing in general on the provocative book by Shepherd Krech, *The Ecological Indian: Myth and History* (1999). In that book, Krech explored the historical validity of the notion that people of Native descent are and always have been conservationists. He reported that Indian people killed many buffalo wastefully, set fires that burned wildly, and slaughtered deer and beaver recklessly. In parts and in whole, Krech found that "the ecological Indian" in American history constituted a myth.

The contributors therefore address broad and deep historical issues regarding the operations of power that have figured and refigured "the ecological Indian." This volume includes essays by Judith Antell, Sebastian F. Braun, Ernest S. Burch, Jr., John Dorst, Harvey A. Feit, Dan Flores, Brian Hosmer, Robert L. Kelly, Stephen J. Langdon, Larry Nesper, Mary M. Prasciunas, Darren J. Ranco, and James H. Schlender. Harkin and Lewis also contribute their perspectives, as does Krech in chapter one and in an afterword. The contributors are a multidisciplinary lot of historians, anthropologists, lawyers, and environmentalists, some of whom are enrolled members of the Minnesota Chippewa, Penobscot, and the Lac Courte Oreilles Ojibwe.

A number of the contributors express disregard for Krech's book. For instance, Ranco opines that Krech represents the kind of scholar "profoundly disengaged from Native communities" (p. 34). Ranco complains that Krech's book "says nothing," because it was not based on an ethical research paradigm to foster community engagement. He reproaches Krech for his attachment to objective norms, dispassionate science, and behaviorist analysis. Likewise, Feit notes that Krech did not consider the importance of Northern Algonquians learning about ecology from other Northern Algonquians. Feit posits that "we cannot determine who taught conservation to whom over the course of the nineteenth century" (p. 72). Whatever the indeterminacy of culture, he indicts Krech for not giving Indian people proper credit for constructing their own kind of environmentalism outside of Eurocentric concepts of science. Kelley and Prasciunas recognize that no one yet knows with certainty what caused the late Pleistocene extinctions, although the climate change argument tends to attract the most support. Burch recommends that the polarizing debate over whether Native Americans were either rational conservationists or rapacious overkillers be abandoned by scholars, because it signifies an irrational inquiry rather than a scientific one.

A handful of contributors endorse the gist of Krech's book, albeit with caveats. Flores surmises that the Lakota Sioux during the nineteenth century knew nothing about the management of natural resources based on modern principles of ecological science. Within their highly competitive social structure, buffalo hunting on the Great Plains facilitated upward mobility among younger males. Although non-Indian hide hunters unquestionably delivered the final destructive blow to the bison herds, the western Sioux bear responsibility, Flores writes, for "executing a Pac-Man-like march westward, devouring one pocket of buffalo after another" (p. 168). To his credit, Flores eschews the biased efforts to make particular individuals or groups look bad rather than explaining what happened in the past with compassion both for the human actors and for the natural world. With a similar display of fairness and balance, Braun applauds Krech's effort to disentangle Indian people from the stereotypes and the mythologies about them. He opines that Native cultures have been manipulated to provide moral authenticity to non-Indians and continue to be reinterpreted according to the desires of the dominant colonial society in North America. Nesper and Schlender add that the images of Indians appropriated for environmentalist messages denote "an exploitive objectification of indigenous people for non-Indian purposes and mostly at Indian expense" (p. 302).

A few of the essays illustrate case studies that go beyond the ones detailed in Krech's book. Langdon appreciates the Salmon boy myth circulating among certain Indian groups in southeastern Alaska. Over the last five thousand or so years, he surmises, the Tlingit populations developed understandings and practices allowing them to sustain their relationships with the natural world. Their outlook on salmon, Langdon believes, strongly insinuates "a powerful intersection of concept, belief, and behavior according to an entirely different framework" than paradigms familiar to the uninitiated (p. 269). Elsewhere, Lewis reviews the case of the Skull Valley band of Goshute Indians in Utah, who contracted to store forty thousand metric tons of high-level radioactive waste for up to forty years on a concrete pad. Acknowledging the ironies of the case, he resolves: "Generalizing about a generalized environmental or conservationist ethos of a generalized Indian population over a generalized past is like trying to nail Jell-O to the wall" (p. 333).

To be sure, the dense prose necessary to explain complicated and nuanced points of view tends to obscure facts rather than to clarify them. To give but one example from this volume, Harkin supposes "that predation and reciprocity are simply different modalities of the same ecological and spiritual vectors, but that the reciprocity modality is marked and the predation modality unmarked" (p. 222). Of course, one wonders precisely how scholars interested in Native Americans and the environment are supposed to unpack such metaconceptual baggage.

Any scholar who has followed the controversy regarding Krech's book will find several of the essays in this volume worth reading. Despite its shortcomings, Krech's book sparked important discussions in scholarly circles and among Native Americans. As Krech admits in his closing remarks for this volume, his book survived the fiery charges of political incorrectness and

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cultural insensitivity. Indeed, the fuming of his critics may be saying more about them than about the validity of the evidence presented in *The Ecological Indian*. Undoubtedly, this sort of "dust-up" will appeal to those scholars across various academic fields seeking heated exchanges on cultural differences and about American Indian ecology. For better or for worse, the contributors to this volume try to rekindle a fire that had almost burned out.

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COMPARATIVE/WORLD

SABINE MACCORMACK. *On the Wings of Time: Rome, the Incas, Spain, and Peru*. Princeton, N.J.: Princeton University Press. 2007. Pp. xix, 320. $35.00.

Sabine MacCormack's elegantly written and erudite book shows how Roman and classical literature provided a framework for comprehending the Inca and Spanish empires and helped to construct the historical experience of encounter, the concept of Peru, and its collective consciousness and identity. For anthropological historians, the message is clear: there is no such thing as an uncontaminated Andean text. But, lest one conclude that the story here is one of imposition and top-down domination, MacCormack shows that Spanish forms of knowledge and understanding also changed and mutated in the process of being used by various Spanish and native writers. As a result, the Andeans became part of world history and Rome and its people became part of their historical experience. In this two-way process, the Roman example helped European audiences recognize the Inca government as an imperial state while simultaneously the Roman Empire became a model and precedent for the Spanish Empire.

MacCormack's chapters show how Roman precepts and histories were employed as resilient, versatile, and useful means with which to think. Notable in chapter one is her discussion of the records of Don Joan de Santacruz Pachacuti Yamqui Salcamaygua, which employ Christian, Spanish, and Andean elements. Iconography and architecture show the same. The church facade in Juli is a synthesis of Roman architectural elements (archs, columns, pilasters with their bases and capitals) and the decorations of local artists. The church of Carabuco contains paintings of classical (Hercules and Apollo), Christian, and contemporary scenes. But, the sinister classical mermaid or siren on the Puno cathedral is reinterpreted as an inspiration to Andean musicians, illustrating MacCormack's contention that classical elements were also changed in their employ. Chapter two shows how the Spanish mode of writing influenced American authorship. Roman antecedents helped Pedro Cieza de León and other Spanish historians of the Incas to recognize their governing hierarchy as an imperial state. Cuzco was equated to Rome. The Inca rulers were likened to Roman emperors who as the noblest and most laudable of ancient founders and legislators gave rise to civilized society.

How classical history also influenced the chroniclers' analysis of the civil wars and the founding and governance of cities are the topics of chapters three and four, respectively. MacCormack emphasizes the city as a focus of patriotism and a sense of belonging and includes the interesting observations that Juan de Matienzo's plan for native *reducciones* (nucleated settlements or towns) comes from Vitruvius and that Felipe Guaman Poma de Ayala's depiction of Andean cities resembles the cities and fortifications depicted in the *Notitia utraque dignitatum cum orientis tum occidentis*. Chapter five traces the European influences in the works of Gonzalo Fernández de Oviedo, Pedro Cieza de León, José de Acosta, and Garcilaso de la Vega, and chapter six deals with translations both literal and conceptual. This discussion continues in the following chapter with MacCormack's analysis of Giovanni Anello Oliva, Juan de Velasco, and Ignazio Molina (on Chile).

The sum is a cautionary tale for scholars all too eager to use the chronicles, despite their contradictions, as literal sources for interpretation. Were "chosen women" who lived in Tumbez akin to Cieza de León's depiction as Roman vestal virgins? Was the Inca state as centralized and omnipotent as commonly thought when Molina and Velasco, admittedly representing provincial views, emphasized the local polity instead of the imperial one?

This book fits into an ever-expanding historiography on and re-evaluation of the chronicles. Its most significant contribution is MacCormack's encapsulating analyses of their authors: Gregorio Garcia, Antonio León Pinelo, Fernando de Montesinos, and Diego Andrés Rocha, who were interested in the earliest history of Peru; Miguel Cabello Valboa, Garcilaso, Bernabé Cobo, and Antonio de la Calancha, who were interested in the growth, development, and fall of the Inca empire; Cieza, Garcilaso, and Acosta, who admired the Inca (versus Pedro Sarmiento de Gamboa, for example, who had a more negative view); the author of the Huarochirí manuscript, Guaman Poma de Ayala, Santacruz Pachacuti, and Montesinos, who integrated Andean history with Biblical chronology; and eighteenth-century writers, like Molina and Velasco, who sought to correct and contradict the negative evaluations of Andean reality. Thus, she shows history as the "teacher of life" (p. 274) and how the Roman past allowed room for contradiction, uncertainty, for not knowing, and for not knowing everything, and thus exercised a flexible influence on the writers and builders of colonial history. MacCormack's book places her capable analyses at the service of her colleagues and our students.

SUSAN ELIZABETH RAMÍREZ
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DAVIKEN STUDNICKI-GIZBERT. *A Nation upon the Ocean Sea: Portugal's Atlantic Diaspora and the Crisis of the Spanish Empire, 1492–1640*. New York: Oxford University Press. 2007. Pp. x, 242. Cloth $99.00, paper $22.95.

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Daviken Studnicki-Gizbert's monograph on Spain's early transatlantic system is outstanding on many counts. It details the operations of the Portuguese "nation" (a kind of enclave) of Old and New Christians, Judaizers, and probably some Jews—mainly merchants and their families but also shipmasters, crewmen, and shipyard workers—established in Spain's European and American dominions over the seventy-year Spanish occupation of neighboring Portugal, roughly 1580 to 1650. New Christians numbered more than half the Portuguese "nation." Opening with a chapter on the background of Spain's maritime expansion in the sixteenth and early seventeenth centuries and the tolerated insertion of Portuguese merchants, the book closes with one on the Portuguese enclave's expulsion and/or absorption around the middle of the seventeenth century. In between, four thoughtful chapters review the formation of overseas Portuguese trading groups in Spain's American colonies mainly at Lima, Cartagena de Indias, Panama, and Mexico City, and their linkages with European entrepôts at Seville/Lisbon and with suppliers at Bayonnne, Bordeaux, Antwerp, and Amsterdam of textiles and other exports to the overseas colonies.

At the monograph's core is a comprehensive analysis of the "dense" Portuguese trading complexes via a canvas of business structures, tactics, and outcomes personalized with profiles of leading merchants who wove together Western Europe, West-coast Africa, northeastern Brazil, and New Spain, shipping Africa's slaves and Europe's manufactures (principally woolens and linens) across the Atlantic to the Spanish "Indies" in exchange for millions of silver pesos/bullion from Potosí and Zacatecas. In other chapters Studnicki-Gizbert covers the formation and fluidity of the Portuguese "diasporic trading communities"; the "merchant house as . . . abode and . . . commercial operation" forming the "fundamental unit of the Portuguese merchant class"; and Portuguese *arbitristas*' proposals for Olivares's hoped-for economic "reform"—omitting of course seriously modifying the *carrera de Indias*—by somewhat freer circulation of goods and specie within Spain's transatlantic system. Shared ethnic identity, endogamous marriage, incorporation of family into the commercial house, and assiduous correspondence were used to maintain business contacts and commercial intelligence. Near and extended families facilitated an apparent "vertical" trading system tying together the provenance of trade goods, shipping facilities, distribution networks and—most important—silver flows across the Atlantic. Indeed, the African slave trade, initiated by Portuguese entrepreneurs, generated for Portuguese *asentistas* more than 100 million pesos. This fortune eased massive Portuguese penetration of Spanish imperial finance via loans to Madrid secured on tax-farming (*alcabalas*, *almojarifazgo*) and the valuable monopoly slaving contracts (*asientos*). For example, Enrique de Andrade, prominent in Seville's Portuguese merchant community along with his father and father-in-law, was part of a "tight-knit group of *asentistas* and merchant bankers" owning tax farms and investments in Seville's African slave trade. Overall, the economic and social data presented reflects Studnicki-Gizbert's imaginative sifting of merchants' voluminous correspondence, account books, and property inventories. Supplementing these are published primary sources and a broad spectrum of secondary publications consulted: altogether an impressive documentary base.

Portuguese commercial penetration superseded Genoese and German interests in the Atlantic's sixteenth-century "multinational" trade but proved short lived, cut short by the Spanish monarchy's loss of the United Provinces, Portugal, and Brazil and by the emergence of merchant groups in Holland, England, and France determined to build national (Studnicki-Gizbert's "institutionalized") economies able to exploit Spain's transatlantic colonies. In the two decades after 1630, proto-nationalist, xenophobic, and sometimes anti-semitic groups in Spain and its colonies vanquished the preeminence of the Portuguese "nation" as embedded in Spain's early transatlantic structures. By 1640 the Portuguese were seen as foreigners and as such were obvious targets: they controlled Spain's "most important trading circuits" via "transoceanic networks." In what one may term the second phase of Spain's Atlantic system, French and English merchants would be more successful.

This wide-ranging study indicates that trade in the Spanish Atlantic system remained fundamentally distributive. In fact it is questionable that Spain in the sixteenth century had "mills, manufactories and shipyards" employing "thousands." Portuguese businessmen, like their Castilian and Basque successors, made little effort to promote domestic textile manufactories aimed at colonial consumers despite the "staggering concentration of wealth in their hands." Instead, like their predecessor Fuggers and Welsers, they privileged investment in government financial and fiscal institutions; Portuguese "diasporic" trading left Spain to provision its convoys to the colonies with goods produced in Europe, not on the peninsula. Moreover, in the present age when imperialism receives grudging admiration and globalism virtual universal applause, historians need recall that the "Portuguese Nation," which spread its representatives globally, pioneered the Atlantic slave trade and sustained it tenaciously for four centuries.

STANLEY J. STEIN
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COLIN KIDD. *The Forging of Races: Race and Scripture in the Protestant Atlantic World, 1600–2000*. New York: Cambridge University Press. 2006. Pp. vii, 309. Cloth $75.00, paper $27.95.

Colin Kidd's new book explores the significance of the Bible and theological thought to the shaping of racial attitudes from the seventeenth century to the present. This is an intellectual history of "the ways in which scripture has been mobilised in the pursuit of certain

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theories of race, ethnic identities, racial prejudices and anti-racist sentiments" (p. 1). His focus, he tells us, is not on power or on the ways in which one race has dominated another. Rather his narrative is concerned with the extent to which encounters with others compelled reinterpretations of scripture. For Kidd, as the title of his book suggests, "scripture has been for much of the early modern and modern eras the primary cultural influence on the forging of races" (p. 19).

This is an erudite and scholarly work, rooted in an extensive knowledge of the writings of preachers, academics, and intellectuals. It builds on Kidd's previous work on ethnicity and nationhood in the Atlantic world, developing the argument that the long-lasting and widespread Protestant commitment to monogenesis acted as a break on the development of racisms. "The construction of race," Kidd maintains, "has been significantly restricted in its articulation and meanings by theological imperatives." Theology had a powerful negative impact since the commitment to mankind's common descent from Adam "drew white Europeans into a benign state of denial, a refusal to accept that human racial differences were, literally, anything other than skin deep" (pp. 25–27).

This thesis is explored in a series of wide-ranging chapters dealing with race as a scriptural problem, race and religious orthodoxy in the early modern period, the Enlightenment and the challenge it posed to scriptural authority, the nineteenth-century crisis of faith, and the forms of racialized religion that emerged subsequently, including interpretations of Mormonism and theosophy. A final chapter runs through a number of black counter-theologies with very brief discussions of James W. C. Pennington, Alexander Crummell, Marcus Garvey, and Rastafarianism alongside other black separatist doctrines. It is impossible to do justice to these arguments in a short review; not surprisingly, the most closely argued chapters are concerned with the early modern period—the heartland of Kidd's knowledge. In arguing that race was not a central organizing concept of intellectual life or political culture in this period, he does not engage with the by now extensive scholarship, most of it coming from the United States, which argues the opposite.

I was particularly interested in his attempt to rescue the Enlightenment from its critics and to argue that the most powerful element within the varied strands of Enlightenment thinking was the attempt to make a convincing case for monogenesis on naturalistic foundations. By the 1770s the Common Sense school of philosophy in Scotland had established its dominance, with the liberal critique of David Hume's skepticism spearheaded by James Beattie in Aberdeen. Beattie linked Hume's racial bigotry with his anti-Christian philosophy and argued that an attack on black humanity was an attack on Christian orthodoxy. He believed that the scriptures explained the racial unity of mankind and that differences between peoples were due to environment. Beattie was one of the chief influences on the thinking of James Stephen, who was to become a key figure in the campaigns against both the slave trade and slavery. My puzzle here is that while Kidd is convinced that those who supported monogenetic explanations of racial difference were free from racial hierarchies, my own work on abolition and emancipation would not support this view. Since racial theories were never exclusively about skin, blood, and bones but also about culture and civilization, the conviction that all men were descended from Adam could be entirely consistent with a conviction that the barbarisms of Africa must be eradicated and that Africans must be taught how to live by their more civilized superiors. A profound belief in the universal human family in evangelical thought sat quite comfortably with deep-rooted assumptions of black and indeed female inequality—and an assurance that the role of white men was to tutor their inferiors. Indeed, the conception of the family with its patriarchal father at the head was critical to the articulation of social hierarchies of class, of gender, of race, and of ethnicity. While I entirely agree that much of Western racial investigation was focused on proving sameness rather than difference, sameness was construed as whiteness. The view that at some point in the future these "others" could reach the level of their mentors did not ensure equality of treatment in the present. While even the most radical and progressive missionaries, for example, sang the praises of the universal family of man, their everyday practices were embedded in racial and gender hierarchies.

In his conclusion, Kidd tells us that he had started the work convinced that modern racism only emerged with the dethroning of biblical authority, that racism was a secular doctrine. In the course of doing the work, however, he became convinced that Genesis could be interpreted as supporting either monogenetic or polygenetic arguments: his earlier conviction was modified, but he maintains the enormous significance of traditions of scriptural interpretation on discourses of race. Kidd is undoubtedly right that these traditions were an element in the making of racial thought, but the notion that race only became an important marker of difference in the nineteenth century and that theology, as he puts it, "trumped race" (p. 167) and acted as a powerful brake on doctrines of racial difference will remain controversial.

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S. D. SMITH. *Slavery, Family and Gentry Capitalism in the British Atlantic: The World of the Lascelles, 1648–1834.* (Cambridge Studies in Economic History.) New York: Cambridge University Press. 2006. Pp. xv, 380. $99.00.

S. D. Smith's wise investigation of three generations of the Lascelles family, later barons and then earls of Harewood, presents an unusual and salutary perspective on the history of both the English landed elite and the British Atlantic world over two centuries. In some ways this is the history of a Yorkshire gentry family that prospered as merchants and then as owners of sugar

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plantations in the West Indies from mid-seventeenth to early nineteenth centuries, but the book is also much more, not least because of its pertinence for twenty-first century concerns, reflected in the attention that the Harewood Trust (holder of much of the evidence on which the book rests) has recently directed to its namesakes' past connections with slavery. Although the study is preoccupied with the Lascelles, others figure substantially in it: the Lascelles' kin in Yorkshire, their business associates around the Atlantic, their West Indian employees, and the slaves laboring on their plantations. At once a contribution to the history of sugar, an illustration of the role of office and patronage in the advancement of economic interests, and an appreciation of managerial practice in British capitalism's growth, Smith's book is also "part of an ongoing re-evaluation of slavery's legacy" (p. 3). In this context, the author brings to light the profound entanglement with slavery from which the Lascelles dramatically profited. He does so from beginning to end, commencing with an account of two British North American merchant families—neither of them the Lascelles—who exemplify his argument that "the exploits of . . . gentry capitalists brought into being a business empire of great complexity, built on slavery and patronage" (p. 42).

Far differently but no less effectively, Smith's concluding chapter, "Between Black and White," offers provocative and understated reflections on the nature of the "intercultural relations" to which race slavery gave rise in the West Indies. Here, without denying the need to analyze some interactions between whites and blacks in the stark terms of resistance or cooperation on the latter's part, Smith objects to the consequent categorization of individualsin the enslaved community as either cooperators or resisters. He insists that evidence is lacking to label people so simply and shows the inadequacy of those categories by presenting three very different histories of interracial unions that reveal how much the multiracial stood "between black and white." And the book's longest chapter uses a painstakingly constructed database of those enslaved on the Lascelles' six plantations on Barbados, Jamaica, and Tobago to anatomize the slave population of the late eighteenth and early nineteenth centuries in ways that simultaneously lay bare the economic calculus of the plantation owners and the immiseration of slaves who experienced mortality rates "among the highest of any recorded human population" (p. 307).

Smith's pursuit of the Lascelles' family fortunes—their making, occasional marring, and long persistence—leads inexorably to the human history of plantation slavery, but along the way it takes in the history of commerce and trade, political faction, and governmental corruption, as well as that of highly personal and regionalized networks of kinship and credit. The Lascelles, as perhaps befit their status among Yorkshire's landed elite, were in Smith's formulation "reluctant planters," gentry-merchants whose extensive mid-eighteenth century fortunes, particularly that amassed by Henry Lascelles (1690–1753), derived from commission trading in sugar and rum, government contracts, the slave trade, and moneylending, not from West Indian landholdings. In the event, it was moneylending, and the vast mortgage indebtedness of several financially distressed Barbadian families to Henry Lascelles's son Edwin (1713–1795), that brought over 27,000 acres of West Indian land into the family between 1773 and 1787. The evidence of the Lascelles' lending practices lead Smith (against received wisdom) to elevate the importance of long-term credit over short in the financing of new sugar-planting in the third quarter of the eighteenth century. He also argues that the Lascelles' successful absentee management—evidence that "slavery remained a viable system in the sense that their estates generated an acceptable rate of return" (p. 259)—was accomplished through a modernizing regime of systematic accounting practices, well-trained managers and bookkeepers, and as careful oversight as distance and absence allowed.

Smith's study, part of Cambridge's series in economic history, repays the careful reading demanded by its intricate depictions of family connections, its byzantine tales of political corruption, and the formidable tables and figures of everything from alphabetical listings of the Lascelles' loans to Kaplan-Meier cumulative survival curves. It is a story at once elusive and straightforward: elusive in that its many disparate components—the rise and fall of families linked to the Lascelles (chapters two and five), the text of the 2nd Earl of Harewood's speech in 1832 advocating the continuation of slavery—might ideally have been drawn together more explicitly in the telling. But it is straightforward in the entirely compelling case it makes for, and the example it provides of, conceiving the history of the British transatlantic nexus as broadly as possible.

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Kate Davies. *Catharine Macaulay and Mercy Otis Warren: The Revolutionary Atlantic and the Politics of Gender*. New York: Oxford University Press. 2005. Pp. xi, 319. $95.00.

In this meticulously researched study, Kate Davies examines two famous revolutionary era historians to better understand transatlantic correspondence, classical republicanism, sensibility, and feminist thought at the end of the eighteenth century. Davies pays close attention to ways in which different social contexts (London and Boston) affected the ways in which vocabularies of republicanism took on a range of meanings. As she does so, Davies offers a model for making sense of transnational and transatlantic intellectual movements without losing sight of local nuances. She also sets new terms by which women's political writing in the 1770s and 1780s might be more fully recognized as an important part of a reconstructed literary canon.

The book's first half traces Catharine Macaulay's intellectual origins, public image, and support for the American Revolution. Davies pays careful attention to

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representations of Macaulay in print, statuary, and portraiture, contextualized alongside images of Britannia and classical Roman matrons. Aligning herself with such figures of patriotic femininity helped mediate the notion of masculine virtue built into ideologies of republicanism, allowing Macaulay to be accepted by the public as a professional writer. But gender inflected the revolutionary politics of reputation in other, sometimes competing ways. During the war, when popular prints showed Britannia and a nude female America locked in a mother-daughter catfight, Macaulay was portrayed by her opponents as falling on the *belle sauvage* side of that dyad. The popular British response to the war, Davies argues, depended on a rending apart of what had previously been "intertwined narratives and languages" of patriotism and republican sentimentalism (p. 178), a combination that had allowed Macaulay to attain her celebrity; once these literary strands were separated, Macaulay was vulnerable to being framed in gothic rather than sentimental terms: as a female grotesque.

The book's second half crosses the Atlantic, first with Macaulay's letters and then with her person, via her friendship with Mercy Otis Warren, a fierce Massachusetts republican in the postrevolutionary era. Warren's epistolary networks during the war allowed American women to influence the revolution from within a conservative embrace of domestic (and rural) retirement. After setting this American groundwork, Davies offers her most compelling chapter, a reexamination of the *Sans Souci* controversy between hard-line republicans like the Warrens and a younger set of metropolitan fashionables bent on reintroducing urban politeness—tea assemblies, balls, gambling, and eventually the theater—into postwar Boston. Here Davies's transatlantic perspective operates to her greatest advantage, allowing her to make more of this event than previous historians have. The event occasioned a misunderstanding between Davies's protagonists that revealed the different meanings each had attached to republican keywords. Macaulay's notions of republicanism did not preclude metropolitan sociability; she subscribed to the new club and was held up by its members as a sign of their own virtuous intent to refine Boston's best citizens. Warren criticized the club in private correspondence and was accused of writing a play satirizing it. She saw institutions like the tea assembly as a waste of time and money and as effeminizing, as counteracting the masculine virtue and rural domesticity republicans endorsed. Davies shows how competing conceptions of women as either refining or dissipating society worked for and against the club in newspaper debates during the winter of 1784–1785.

Though the two women mended their friendship, the tensions present in this affair persisted through the turn of the century, eventually rendering Warren a political outsider. But Davies aims to do more here than outline the contours of a single friendship—even if that friendship was central to a transatlantic correspondence that was important to the American Revolution itself. She aims instead to make each woman stand for a wide swath of revolutionary culture on both sides of the Atlantic. One central claim structures this effort. Countering J. G. A. Pocock's argument that modern feminism derives from mercantile liberalism rather than classical republicanism (which he reads as unequivocally misogynistic), Davies argues that Macaulay and Warren understood themselves as republicans precisely via their association with traditional femininity. (This point seems to anticipate more challenge from British than American historians, the latter having long since accepted republicanism's and female patriotism's union in the ideology of republican motherhood.) In Davies's model, the culture of sensibility is not at odds with republicanism; rather it provides a vocabulary from which women like Macaulay and Warren could make republicanism their own.

Davies makes this central point persuasively, even if the book remains bound to biography more fully than she intends. The fact that her protagonists are both female also leads her, perhaps, to overemphasize the work of *female* friendship; the book's own narrative reveals that both writers maintained influential correspondence networks that were mixed sex. But such small objections aside, Davies's work should be essential to any future effort to trace the transatlantic careers of literary and intellectual forms associated with gender, republicanism, and revolution.

Bryan Waterman
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Edward Watts. *In This Remote Country: French Colonial Culture in the Anglo-American Imagination, 1780–1860.* Chapel Hill: University of North Carolina Press. 2006. Pp. ix, 275. Cloth $59.95, paper $19.95.

Rafe Blaufarb. *Bonapartists in the Borderlands: French Exiles and Refugees on the Gulf Coast, 1815–1835.* Tuscaloosa: University of Alabama Press. 2005. Pp. xix, 302. $50.00.

Burgeoning interest in early modern France's colonial enterprise is enhanced by these two recent studies, notwithstanding the authors' noticeably different approaches to historical explanation. From the vantage of cultural history, Edward Watts explores how antebellum Anglo-American authors imagined a French social, political, and cultural presence in the Mississippi Valley. Representations of trans-Appalachian French settlers were critical, Watts proposes, both to the depiction of emerging racial, economic, gendered, and nativist boundaries of national identity and to their contestation in the antebellum United States. Watts's suggestion that representations of the French frontier assume increasingly ideological weight in antebellum American writing is certainly intriguing, although the argument is at times diffuse. "This book," he states, "is about how competing ways of imagining the nation were contested and entangled in epitome through contrasting narratives and representations of the French frontier and the

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question of its relevance for antebellum America" (p. 4). Watts identifies and analyzes underlying patterns in the literary representations of the former French borderlands, drawing from an array of printed English-language texts, including local histories, works of fiction, legal texts, travelers' accounts, and reports of government-sponsored expeditions. His readings document two distinctive modes in which francophone frontier residents were portrayed: a dominant vision elaborated by "Anglo empire builders" and alternative counter-narratives written by "dissident Anglo writers" (pp. 5, 4). Proponents of a racialized, commercialized, anti-Catholic patriarchal vision of nationality relegated French settlers to the prehistory of the republic, criticized the backwardness of open-field agriculture practiced by French peasant cultivators, disapproved of French frontiersmen's cohabitation and intermarriage with Amerindian women, and repeatedly disparaged the indolence and impoverishment of French settlers "doomed to vanish before Anglo-Saxon inevitability" (p. 110). A contrasting vision of national identity celebrated French settlements for their cultural pluralism, mutualism of intercultural contacts, small-scale mixed farming, and the ending of slavery through peaceful manumission. In these ways, Watts argues, antebellum accounts of French frontier life anticipated the terrain on which racist and nativist boundaries of exclusion would be drawn in the 1890s: "How the colonial French—with the Irish, the first non-Anglo ethnic group encountered by post-Revolutionary Anglo-Americans—were written into or out of the nation's history, how they were rejected, accepted, assimilated, and, most important, absorbed, served as a template for how 'native-born' Americans would understand or control their relationship with the increasing population of non-Anglo white immigrants throughout the rest of the century and beyond" (p. 189).

While Watts's valuations of these two modes of representation are clear, the conditions that make their cultural production possible seem underspecified. It is not always apparent who is doing the imagining, how and where various ideas circulate, or how they are received. Nevertheless, Watts's often original interpretive readings of particular texts establish his observation that "Virtually every account written in English of traveling, trading, pioneering, hunting, settling, or surveying on the early nineteenth century frontier is saturated by men with French names" (p. 95). Their representation, he argues, facilitated "a consolidation of national identity taking place in the early nineteenth century that found its model and precedent in the justification of the removal, assimilation, and displacement—in sum, the erasure—of the French in the eighteenth" (p. 41).

Rafe Blaufarb reconstructs the socioeconomic and political trajectories of French settlers in west Alabama's Vine and Olive colony. The arrival in the United States of some 30,000 francophone immigrants after Napoleon Bonaparte's defeat at Waterloo becomes occasion for close comparisons of their material and imagined presence. Hence, Blaufarb's book skillfully distinguishes groups of French migrants made visible by conventions of post-Reconstruction mythmaking from other, more invisible groups of francophone immigrants who resided in the United States during the first quarter of the nineteenth century. The range of interpretive horizons opened by this ambitiously conceived monograph is stunning. In the context of this review, Blaufarb suggests that the antebellum accounts examined by Watts, whether they celebrated or condemned the traditionalism of backwoods French peasant pioneers, might well have flourished at a moment when new groups of urbanized, commercially oriented bourgeois landowning families and exiled Bonapartist military officers were fundamentally reconfiguring the francophone immigrant population in the United States.

Impressive, wide-ranging research in diplomatic and military archives in France, Spain, and the United States, combined with close consultation of French-language newspapers, family correspondence, and genealogical collections in regional and state archives, underlies Blaufarb's deft identification of distinct if overlapping subcultures among early nineteenth-century French immigrants. First, the migration experiences of refugees from Caribbean slave revolution "give lie to the myth that the Saint-Domingue revolution was a 'nightmare of gore' in which the colonists had to flee precipitously, with just the clothes on their back, from ravening hordes of bloodthirsty ex-slaves" (p. 24). Not only did many Saint Domingue refugees manage to leave "in a relatively orderly fashion, with at least some of their moveable property," he argues (p. 24), but post-migration marriage strategies and old transatlantic commercial ties were key elements in the success with which, in cities along the eastern seaboard, in New Orleans, and as far west as St. Louis, some Saint Domingue refugees reconstituted lives as importers of Haitian and French commodities. Next, Blaufarb distinguishes two factions among a group of several hundred French expatriates in Philadelphia who benefited from the strategic considerations "to secure America's hold on the Gulf Coast, especially Spanish Florida," which in March 1817 led Congress to allocate to the immigrants' Vine and Olive colony some 92,000 acres from lands recently conquered from the Creek Indians (p. 51). It was, he argues, Saint Domingue families of "former coffee cultivators, merchants, craftsmen, shopkeepers, and clerks" who in largest numbers actually took up their southern allotments, able "at last [to] achieve in Alabama the planter status denied them—but long envied—in Saint-Domingue" (p. 119). Close analysis of rumors and reports that traveled along the networks of communication of diplomatic bureaucracies brings into view the activities of a second Vine and Olive contingent: Bonapartist military and political exiles and restless young men whose prospects for a military career had been ended by Louis XVIII's reforms of the French army. Few of these former Bonapartist officers or soldiers-on-the make, he notes, moved to Al-

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abama. Instead, Blaufarb's investigations of their connections to a third, little explored transatlantic underworld of military adventurers, mercenaries, professional revolutionaries, smugglers, and privateers brings into view global dimensions of independence movements in Spain's American empire. By 1815, he argues, conflicts in Spanish America "attracted to the United States an international community of revolutionaries, mercenaries, and scoundrels whose plots and exploits threatened to upset the delicate calculations and negotiations of the established powers as they maneuvered to gain an advantage from the breakup of Spain's empire" (pp. 61–62). Blaufarb's inventive designation of a geopolitical "Western Question" locates in Spain's crumbling American empire the stimulus to "a fundamental reordering of international power relations in the Atlantic world" (p. xvi). Although the ill-fated Champ d'Asile invasion of Texas that was mounted by a few veterans of Bonaparte's imperial campaigns failed miserably, the name itself gained cultural potency in France as a symbol of political opposition. This study navigates gaps between structures of action and representation as deftly as it explores mediating linkages among local, national, and transnational histories.

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Tangi Villerbu. *La Conquête de l'Ouest: Le récit français de la nation américaine au XIX^e^ siècle.* (Histoire.) Rennes: Presses universitaires de Rennes. 2007. Pp. 306. €22.00.

"One could say that the West is the part of America that is most distinctly American. The West constitutes the place where the traits that distinguish America from Europe appear in full relief," wrote the Englishman James Bryce in 1888 (p. 67). His words embody the central argument of Tangi Villerbu's richly researched study of French attitudes toward the American West across the long nineteenth century. Participating in the recent trend toward internationalizing American history, Villerbu argues that American identity emerged in part from foreigners' perceptions, and that the West formed a privileged site for the construction of national identity.

Transatlantic in method as well as topic, Villerbu situates his approach at the juncture between the New Western history and French historians' discursive analysis of representation and memory. He illustrates how French narrators took part in developing some of the most prominent and mainstream narratives of "manifest destiny" as the embodiment of the American spirit. In a fascinating discussion of California, for example, Villerbu shows how French authors and travelers depicted the stages of the "Americanization" of this region. French observers used racial and sexual discourses to define California's conquest and the eventual development of "order" as the epitome of American colonization of the West by white men. Many French accounts characterized Mexicans as colorful and artistic people but disorganized and degenerate administrators. "The decor is superb, but action is slow," wrote George Sauvin about the Mexican era in his five-stage depiction of California's evolution. In his version, the Gold Rush introduced a world of racial chaos, criminality, rampant speculation, and prostitution. Finally, as more white Americans of Northern European descent poured into the region, they established justice and order. In Sauvin's words, "After a stormy childhood, a vigorous and solid State emerged, having the exaggerations of youth, but also the energy. . . . The era of mining gave way to an agricultural and commercial period. The city [of San Francisco] took shape. . . . A number of women came to town, and thanks to them . . . churches opened" (p. 153). Youthful, masculine energy amid ethnic disorder paved the way for a calmer and whiter world of families, workers, and business elites.

Interestingly, Villerbu also analyzes alternate French accounts of how the West was won. For example, Catholic missionaries critiqued the evils of American Protestantism and idealized the conversion of Indians in the days of New France. Even in the nineteenth century, some imagined utopian communities of Jesuits and Indian converts in the Rocky Mountains, but these visions eventually became nostalgic holdovers without staying power as the French missionaries devoted their attention to French colonies in Africa or Asia. In the 1860s to 1880s, the wildly popular French novelist Gustave Aimard promoted a more widespread alternative image: his novels romanticized a pre-Anglo West, an adventure-filled world of Spanish and French trappers and Native American allies or enemies playing out their destinies in a fluid space that resembled Richard White's "middle ground."

Although Villerbu focuses primarily on French accounts, he sometimes examines how Americans, most notably James Fenimore Cooper and Buffalo Bill, contributed to creating French popular myths about Indians and frontiersmen. Cooper's novels resonated with older European images of "noble savages" but tamed those Indians and situated them safely in the past, as part of French fantasies about a lost West. Building beyond Cooper's imagery, French authors wrote westerns in the 1830s to 1850s that reinforced and harkened back to older European imaginaries of America as a space of regeneration and possibility, where French actors as well as Anglo ones could gain riches or self renewal. "The West is once more a place that put into play European national identities, as well as American ones," comments Villerbu (p. 106).

This book only infrequently takes up the issue of how French attitudes toward the United States contributed to constructing French identity. Also, with the exception of judicious analysis of French intellectual milieux of geographers and historians, Villerbu pays surprisingly little attention to how events or trends within French politics or society may have influenced evolving attitudes toward America. He offers tantalizing suggestions. For example, he notes that western images of

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hunters and trappers may have held special resonance for the French facing a crisis of masculinity in the aftermath of the revolutionary and Napoleonic period, but French and American desires for masculinity via the West remain undifferentiated in his analysis. Villerbu also sidesteps an obvious and very current issue: why the French have such a long-term love-hate relationship with the Americans. If the French work in this book to create American identity much more than their own, Villerbu has nonetheless written a rich and welcome example of situating American history in a more international context. His multilayered interpretations of French texts widen the scope of Western history and make compelling reading.

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Louis Sicking. *Frontières d'Outre-Mer: La France et les Pays-Bas dans le monde atlantique au XIX^e siècle*. Paris: Les Indes Savantes. 2006. Pp. 207. €28.00.

Historians of European colonialism typically focus on Britain and France rather than smaller colonial powers such as the Netherlands. To redress this imbalance, Louis Sicking examines three sites of Franco-Dutch conflict: the Caribbean island of St. Martin; the Gold Coast (Ghana); and the border dispute between Surinam and French Guiana. In these regions projects for the extension of French territory at the expense of the Netherlands were proposed and supported by colonial and naval officials along with various special interest groups. In his painstaking examination of diplomatic correspondence (drawn primarily from the Archives d'Outre-Mer), Sicking illuminates the complex and often contradictory motives of those involved and the means by which the Quai d'Orsay and The Hague restrained efforts at territorial aggrandizement.

The Dutch and the French both settled St. Martin, eventually reaching an agreement to split the island in 1648. This division was not revisited until the early 1840s, when the French navy became intrigued by the Dutch port of Philipsburg, which could harbor large vessels and counterbalance the British naval presence in the region. Although a plan to annex the Dutch side of the island was forwarded to the Quai d'Orsay, the idea was rejected on the grounds that such an acquisition would add a large number of slaves to the colonial population and thereby raise the specter of emancipation, an issue that the July Monarchy was unwilling to countenance.

The prospect of annexation would arise again once slavery was abolished following the Revolution of 1848 and the establishment of the Second Republic. Sugar planters complained they were now bereft of a labor force as former slaves migrated to the Dutch side of the island to work in the more lucrative salt mines. In 1853 they submitted a petition to this effect and were supported in their efforts by the territorial governor, who believed that annexation was possible in exchange for the cost of all Dutch slaves. While naval and colonial officials were favorably inclined, the Quai d'Orsay balked as it believed the Dutch would soon abolish slavery, thereby rendering the plan moot. Although they were incorrect—the Netherlands did not end the practice until 1863—the project was abandoned and the division remains to this day.

It was not sugar or salt but the promise of gold, ivory, and the burgeoning trade in slaves that drew European voyagers and adventurers to the West coast of Africa. Indeed, once slavery surpassed gold as the chief export, Portuguese, Dutch, British, and Danish merchants built fortified trading stations along the Gold Coast. By the mid-nineteenth century, however, only the British and Dutch remained.

To facilitate trade the two powers concluded a treaty in 1867 that awarded the eastern part of the coast to the British and the western part to the Dutch. As a result, the new Dutch holding of Apollonia was contiguous with the Ivory Coast. As in St. Martin, French naval officials played a role in the events that followed as they wanted to lay claim to the former French fort of Amackou, then exchange the fort for Apollonia, thereby expanding French territory. Officials in Paris were hesitant about the unstable situation in the region, however, and the matter was closed once it was determined that the claim could not be substantiated. Given the violent reactions of various African peoples to the Anglo-Dutch treaty and the eventual civil war in Apollonia that followed, such misgivings were well placed. Indeed, the Dutch vacated the Gold Coast in 1871–1872.

Boundaries were also ill defined between Surinam and French Guiana, particularly in the forest hinterlands. While the Maroni River was the basic divide, a border dispute arose following the discovery of gold by French prospectors in the territory between the Tapanahoni and Lawa rivers in the 1880s. Demands for mining concessions ensued and upon Dutch insistence the dispute was sent to arbitration and eventually mediated by Tsar Alexander III. Although the Quai d'Orsay believed the tsar would rule on its behalf, the matter was settled in favor of the Dutch with the proviso that all previous concessions granted by the French government be recognized by Surinam.

This was a victory for the Dutch, who were not interested in the auriferous territory (the costs associated with exploration were prohibitive and greatly inhibited mining efforts in the region) but rather in demonstrating that they remained a viable colonial power. If they had simply acquiesced and recognized French claims, this would have damaged Dutch colonial prestige and set a precedent that could endanger their hold over Indonesia. Indeed, Dutch diplomats viewed the East India colony and events in South America as inextricably intertwined.

This is a sophisticated account of the role played by various pressure groups in colonial affairs and their sometimes troubled relationship with metropolitan officials. In this regard, Sicking finds striking similarities in these seemingly disparate affairs, particularly be-

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tween events on the Gold Coast and the St. Martin question. In sum, this is a well-researched examination of an understudied subject.

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GERALD HORNE. *The Deepest South: The United States, Brazil, and the African Slave Trade*. New York: New York University Press. 2007. Pp. v, 341. Cloth $75.00, paper $24.00.

Former Brazilian President Fernando Henrique Cardoso remembers U. S. President George W. Bush asking him, late in 2001, "Do you have blacks in Brazil?" Gerald Horne's book is for persons who know the answer to Bush's question but are less aware of the U. S. role in bringing captive Africans to Brazil in the nineteenth century and dealing with the large South American slave power.

Horne draws together several strands of the story of how U. S. nationals dealt with, and thought of, Brazil during the eighty years before abolition came to that country in 1888. At the core of each strand is either the slave trade or slavery. In the 1830s, slave-based agriculture was expanding in the U. S. South at the very time opposition to human bondage was growing in other parts of the country—and beyond. In states where slavery was outlawed and abolition movements were springing up, American investors, ship builders, manufacturers of trade goods, and shippers profited from the fact that slaves were steadily being acquired along Africa's tropical coasts and transported to Brazil for sale. Helping this along was the U. S. government's refusal to allow British patrol ships to search suspicious vessels flying the American flag. Once gold was discovered in California and shiploads of Americans headed for California by sea around Cape Horn and stopped in Rio de Janeiro, interest in Brazil heightened, especially among slavery's supporters. Some southerners—Horne focuses on two Virginians, Governor Henry Alexander Wise and oceanographer Matthew Fontaine Maury—considered Brazil as a possible spot to relocate with human chattel and, as the Civil War approached, entertained notions that a strong trade relationship with Brazil might hold off abolitionist pressures from the northern states and Great Britain. With the war's end and emancipation, Brazil became a place of retreat for former Confederates, who could not abide life in the United States on a theoretically equal footing with their former slaves. Unable to bring their human chattel with them, most of these expatriate "*Confederados*" faced disappointment and returned. The few who remained in Brazil grew bitter as they fought abolition in their new homeland.

A quick, initial look at this book elicits excitement over its potential. Horne examines U. S. involvement in nineteenth-century slave trading and hemispheric relations from a fresh, Atlantic perspective; he bases the study on research in archives and libraries around the Atlantic basin; and he has a record of scholarly achievement rarely matched.

But a careful reading brings frustration and disappointment. The frustration comes largely from the way the book is written and presented, and for this New York University Press bears considerable responsibility. Much of the text consists of Horne's piecing together of the words of others. He quotes and quotes, often when his own words would serve him better: primary and secondary sources, published and unpublished sources, the latest authorities alongside old and obscure dissertations and papers. He gives them equal authority, as if being in print in itself makes each one valid. Moreover the author rarely notes in the text whose words he is quoting, so the reader must turn constantly to the endnotes, only to encounter a welter of cryptic citations ("Ibid." in this book apparently means merely "cited somewhere earlier") that can force one to invest hours, literally, searching back over hundreds of citations on scores of pages to determine a quotation's authority. A bibliography would ease this process, but there is none.

The disappointment is fueled by Horne's lack of care in using information from his sources. He takes statements far out of context to support his arguments. An example occurs in chapter two, which begins with a statement, essential for Horne's argument, about how "U. S. nationals were leaders in fomenting the illicit [i.e., nineteenth-century] slave trade" (p. 33). Quoting J. E. Inikori ("The Struggle against the Trans-Atlantic Slave Trade," in Sylviane A. Diouf, ed., *Fighting the Slave Trade: West African Strategies* [2003]), Horne writes that U. S. nationals, "acting alone or in conjunction with the bandits, intervened in the affairs of these [African] chiefdoms to provoke conflicts that generated export captives." When one examines Inikori's quotation (which comes not where Horne's citation indicates, on pages 170–171 of the essay, but on page 182) to seek better understanding of the seemingly thorny matter, one finds that Inikori is not writing about Americans in the nineteenth century, but about Europeans in the seventeenth century. This leaves the reader wondering how many more of the author's arguments are based on such misrepresentation of the words of others that he uses so extensively.

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LISA CHILTON. *Agents of Empire: British Female Migration to Canada and Australia, 1860s–1930*. (Studies in Gender and History, number 30.) Buffalo, N.Y.: University of Toronto Press. 2007. Pp. viii, 240. Cloth $60.00, paper $27.95.

British philanthropic women's entrepreneurship in women's emigration from the late nineteenth century through to the 1920s is a rich topic that has at last found its historian. While other scholars have dealt with the

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British Women's Emigration Association (BWEA), and the emigration work of the Girls' Friendly Society and the Young Women's Christian Association (YWCA) and several other groups, Lisa Chilton's monograph is the first to focus specifically on these organizations, and the topic of British women's emigration in this later period, as opposed to the different forms of women's migration in the mid-nineteenth century. As Chilton demonstrates, it is a substantial topic. Tens of thousands of single women emigrated from Britain to the white-settler dominions (and elsewhere) during these decades. Even as we continue to grapple with the social and cultural transformations wrought during first-wave feminism, we are still learning about the dimensions and significance of women's greater mobility: their mobility within urban environments, from hinterlands to cities, and their international travel and migration. Chilton's focus is primarily on the middle and upper-class women philanthropists and reformers who turned themselves into managers of imperial migration. She argues that we need to recognize the extent of their work, its central role in the large-scale migration from Britain in these decades, and the ways in which such work was pro-women if not explicitly feminist, as well as, more obviously, imperialist. The book is concerned with migration to Canada and Australia in particular, although there are recurrent references to other destinations such as South Africa.

A. James Hammerton pioneered the subject of women's migration within the empire three decades ago, and work in the field must still be seen as building on his. Hammerton's focus was on the emigration of single middle-class women: who they were, their social position, their experiences of migration, and the reformers who helped them, especially the Female Middle-Class Emigration Society, which operated from 1862 to 1886. Chilton's focus is later, on the subsequent organizations, and the work, writings, attitudes, and positioning of the women "emigrators." An important book in this area is Jan Gothard's study of the working-class women who were assisted to migrate to Australia to become domestic servants between 1860 and 1900, their experiences of the voyage and their early reception. Again, Chilton's work differs in its focus on the women philanthropists who funded, promoted, organized, and ran women's migration. In explicating middle-class and elite women's pro-imperial attitudes and activism, the book complements recent studies in that field by Katie Pickles and Julia Bush.

Chilton shows the rise of the new generation of women-run bodies concerned with women's migration from the 1880s, their popularity during the period of jingoist imperialism either side of the South African War, and their work during World War I. She discusses the conundrums surrounding the transition from voluntary-run emigration to the state takeover in the 1920s under the auspices of the Society for the Oversea Settlement of British Women (SOSBW): that is, the ways in which the organizations' success led to their demise. Sections of the book employ the methods of cultural history, such as Chilton's analysis of the "narratives of safe passage" that emigrators deployed during the fear of sexual dangers for women that was at its height during and after the 1880s.

While Chilton invokes both transnational and comparative perspectives, the book is more the former than the latter. Its vantage point is mostly British, with occasional shifts to Canadian and Australian locations. It connects Britain, Canada, and Australia in the one appropriate framework, making the case for such transnational studies, but it is not an explicit or detailed comparison of the Canadian and Australian cases.

Australian historians will particularly welcome chapter six, an original and intriguing case study of the organized migration of British women to become domestic servants in the fledgling Australian national capital of Canberra. A planned city purposely located between the major cities of Sydney and Melbourne, Canberra's slow development between Australian federation in 1901 and the arrival of the Commonwealth parliament in 1927 was fraught by several tensions. The Federal Capital Commission that oversaw Canberra's development became convinced that its parliamentarian, diplomatic, and public servant residents were in particular need of domestic servants. In 1926–1927 the commission worked with the SOSBW to import a special group of domestic servants from Britain and built a hostel to accommodate them. The scheme was successful but limited and short lived. It ran into a wall of criticism, in the press and the federal government, partly due to resentment of Canberra's privileged status, and partly due to nationalist concerns that Australian women workers were being passed over in favor of those from the imperial metropole.

Chilton's book will be welcomed by British Empire historians and Australian and Canadian historians, as well as by historians of migration, of philanthropy and, not least, of women.

ANGELA WOOLLACOTT
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ANTHONY BRUNDAGE and RICHARD A. COSGROVE. *The Great Tradition: Constitutional History and National Identity in Britain and the United States, 1870–1960.* Stanford, Calif.: Stanford University Press. 2007. Pp. xii, 341. $65.00.

From the title of Anthony Brundage and Richard A. Cosgrove's new book I anticipated a treatise on the evolving constitution and its link with national identity. Instead the authors tackle the historiography of the field. It is always unfair to fault authors for not writing the book one wants, and as a historian of constitutional history I certainly applaud their attempt to discover why a subject once at the center of the history curriculum should have been relegated to a quiet corner of the historical enterprise. The book under review sets out to determine how constitutional history rose to pre-eminence and the trends within the academic and political world that have consigned it to obscurity. In the process

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the authors aim to explore the link between constitutional history and the national identity of Great Britain and America. So far, so good.

The book is arranged thematically. Chapters dealing with broad themes such as national identity, race, the transformation of historians from storytellers to close readers of records, the impact of empire, and the uneasy relationship between law and history alternate with those devoted to the work of particular practitioners, some, such as John Horace Round, thoroughly nasty bigots. It is all interesting if also often confusing.

The chapters on the changing and varied understanding of the Whig interpretation of history and on race and empire are especially interesting. By contrast, thematic chapters unavoidably result in much hopping about in time and are often hard to follow. Further, while examples are important it is not clear why particular individuals such as Round deserve as much space as an entire field, such as legal history. Still, historical trends of all sorts are dealt with and the work of scores of leading historians from the 1870s to 1960 placed in context. The authors have combed some seventy collections of historians' papers to ferret out their personal views of their field and each other. There is a great deal on offer.

The book's shortcomings result from the effort to embrace so broad and amorphous a subject, a subject never sharply defined. For example, while one chapter is devoted to American constitutional history and American historians are plunked dutifully, if briefly, into other chapters, the book is almost solely about the English tradition and English and/or British national identity. The American aspect seems an afterthought. The problem of national identity, of course, remains important for Great Britain as it attempts to meld the Welsh, Scots, and Irish peoples and cultures into a coherent sense of Britishness. Historians not only of constitutional history but of social, cultural, economic, political, and religious history have had difficulty finding a way to deal with that diverse national base.

Further, there is little attempt to demonstrate the actual impact of historical writing on the views of Britons and Americans of each era. This is a difficult task but more information on popular culture would have helped make the necessary link and enlivened the text.

Since the authors, experts on nineteenth and twentieth century historiography, are interested in what historians of that period wrote about an era rather than what contemporaries of an earlier period thought about their national identity, they occasionally make mistakes. For instance, after discussing the radical political implications of a focus on the Anglo-Saxon village council as opposed to Norman feudalism, they conclude that such radical implications are the reason the Whigs in 1688 "chose ... to anchor their cause on Magna Carta and later parliamentary restrictions on royal power, rather than on the more egalitarian, if more ancient, practices of village councils" (p. 1). Although nineteenth-century historians found the Anglo-Saxons a key to the English constitutional tradition, few pamphleteers of the seventeenth century dwell on these Anglo-Saxon village councils.

The chapter on law and constitutional history paints a bleak picture of the different approaches to the subject by nineteenth-century lawyers and historians. The authors see a separation into lawyers' history and historians' law, with lawyers shunning archival research and only Frederic William Maitland, who practiced law before turning to historical research, successfully bridging the gap. Given the increasing numbers of students earning both law degrees and history doctorates, this division should blur.

To conclude: why did constitutional history rise and fall? Brundage and Cosgrove argue it rose with the academic profession of history in the 1870s and began its decline after World War I as interest shifted to the origins of that war, to politics, biography, and social history. The loss of empire furthered the trend. Happily legal and constitutional materials have proved useful to other historical fields, and the authors see constitutional history making something of a comeback. With this solid historiographical beginning I urge Brundage and Cosgrove to consider writing a book on the constitution and national identity.

JOYCE LEE MALCOLM
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STEPHEN BROADBERRY. *Market Services and the Productivity Race, 1850–2000: British Performance in International Perspective*. (Cambridge Studies in Economic History.) New York: Cambridge University Press. 2006. Pp. xix, 409. $95.00.

The story of modern economic growth, as traditionally told, is the story of modern industry. It was in industry where historically unprecedented rates of growth of output per worker were first witnessed in the early nineteenth century. It was in connection with industrial applications that the key technological innovations were developed. It was in industry where elaborate managerial hierarchies and an extensive division of labor were widely utilized. Debate persists, students are told, about the causes of these developments, but there is little debate about their location. Arnold J. Toynbee did not call it the industrial revolution for nothing.

The subsequent story of comparative economic performance is similarly told in terms of industrial prowess. The United States overtook Britain in output and productivity because of its success in harnessing abundant natural resources to a manufacturing industry that produced standardized products for an extensive home market. Germany overtook Britain by deploying a banking system capable of mobilizing large amounts of capital for investment in heavy industry. Japan overtook Britain because of a culture of cooperation and a single-minded focus on economic growth that enabled it to become a leading exporter of high-quality manufactures. Britain failed to match them because institutions and attitudes inherited from the past were ill suited to circumstances of twentieth-century industry.

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Stephen Broadberry's work constitutes the most important challenge to this conventional view. Broadberry's earlier book, *The Productivity Race: British Manufacturing in International Perspective, 1850–1990* (1997), carefully reconstructed the quantitative record of British, German, and American manufacturing. Its surprising finding was that productivity in U. S. manufacturing was about double that in British manufacturing in both 1870 and 1990. Productivity in British and German manufacturing was about equal in both 1870 and 1990. Thus, there was little change in relative position over the period. Yet the slow growth of the British economy, in comparison with those of the United States and Germany, remains an incontrovertible fact. By implication, the roots of the problem lay elsewhere, outside industry.

This second installment takes this revisionism another step. Broadberry provides new estimates of productivity growth in market services (that is, services provided by for-profit entities, not the public sector). These show that the relative decline of the British economy was, in an accounting sense, due to stagnant productivity in transport and communication, distribution and finance, and professional and personal services. It was in these sectors that Britain failed to innovate and adapt: to adopt the hierarchical managerial structures of the Chandlerian firm and the extensive division of labor made possible by the typewriter, the vertical file, and other modern office equipment. Some British firms, producing services that could not be traded across borders, were sheltered from the chill winds of foreign competition. Others, having grown up in a simpler organizational and technological era, were wedded to old ways. Relatively low levels of general education and militant trade unions able to resist the intensification of the labor process further obstructed the "industrialization" of services. These points are established through a combination of careful statistical analysis and detailed case studies.

At one level, all of this is quite convincing. Circa 1870, Britain was the world's most efficient producer and exporter of financial services, insurance services, shipping services, and accounting services. Over time, however, its lead in these sectors was erased or was even overtaken by German and, especially, American producers quicker to reorganize so as to capitalize on new technologies. Indeed, it was outside manufacturing—in transport (the railways), distribution and, of course, the army—that the Chandlerian line-and-staff form of management, with its extensive division of labor, was first elaborated. Only from there did this set of organizational innovations spread to industry. Broadberry's insightful analysis reminds us of these important facts.

Yet some of these conclusions rest uneasily with what we think we know. London, not New York or Frankfurt, is still the world's leading financial center and, by implication, producer of financial services. Anyone who has attempted to get a plumber on a weekend will be startled to learn that Germany is a significantly more efficient producer of professional and personal services, by a margin of twenty percent.

Other questions suggest themselves. Is the author's focus on private services driven by the even greater difficulty of measuring productivity in public services, where inputs and outputs are even harder to distinguish empirically? Other work, by Nick Crafts for example, suggests that Britain's productivity problem, especially after 1945, lay in public services and was greatly aggravated by nationalization. And how different would the picture look after 1990, when a new generation of information technologies, particularly applicable to services, appears to have repealed the iron law that productivity ineluctably rises faster in manufacturing?

Broadberry's book will also be thought provoking for scholars concerned with current economic developments. In the last ten years, when productivity has grown significantly faster in the United States than in Europe, U. S. out-performance has centered in three service sectors—finance, retailing, and wholesaling—where American firms have reorganized their operations to capitalize on the efficiency-enhancing opportunities afforded by new information and communication technologies, but where their European competitors have been slower to adapt, owing it is argued to a less conducive institutional inheritance and a more restrictive regulatory environment. Broadberry takes a brief stab at the British case toward the end of the book. But a fuller treatment of the parallels between Britain in the past and continental Europe today might usefully be the subject of his third volume.

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MATTHEW HOFFMAN. *From Rebel to Rabbi: Reclaiming Jesus and the Making of Modern Jewish Culture.* (Stanford Studies in Jewish History and Culture.) Stanford, Calif.: Stanford University Press. 2007. Pp. x, 292. $60.00.

Jesus, as we all know, was Jewish. This did not make medieval Jews any fonder of him: they expressed their traditional hostility in uncomplimentary sayings and tales. In the late eighteenth and nineteenth centuries, the *maskilim* ("enlightened" Jews) took a different attitude, and the theologians of the German Jewish Reform movement, inspired by the Christian search for the historical Jesus, wrote about his ideas, personality, and heritage. Although they quibbled about the details, an array of nineteenth-century Jewish intellectuals argued that Jesus' most attractive notions were Jewish not just in origin but in essence. Implicitly, then, Jews who found themselves drawn to Western (Christian) culture were merely reclaiming their own.

At the end of the nineteenth and beginning of the twentieth centuries, the modern Jewish fascination with Jesus inspired a wave of Yiddish (and sometimes Hebrew) stories, poems, and novels, as well as statues and paintings. Jewish intellectuals debated the propriety

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and the politics of Jewish depictions of Jesus, which flourished in spite of some critics' disapproval. The martyrdom of Jesus became a powerful, painfully ironic metaphor for Jewish suffering at Christian hands in Eastern European pogroms in 1903–1907 and even more so during and after World War I. With the genocide of World War II, though, Jewish artists—with the two significant exceptions of Sholem Asch and Marc Chagall—moved away from depicting Jesus.

In his intelligent and well-written monograph, Matthew Hoffman is the first scholar to juxtapose the treatment of Jesus by *maskilim*, Yiddish writers, and Jewish visual artists. In the generous space devoted to the Yiddish literary material, he gives the English-language reader access to a sometimes brilliant and often shocking assortment of texts, many never translated. Although scholars have touched on the interest in the crucifixion among Jewish modernists—most notably David G. Roskies, in *Against the Apocalypse: Responses to Catastrophe in Modern Jewish Culture* (1984)—Hoffman's is the first sustained examination of the topic. He tells a compelling story about the rise and fall of Jewish interest in Jesus. He is undoubtedly correct in arguing that Jews wrote about Jesus both in order to defend Jewish assimilation into Western culture and in order to "re-Judaize" that target culture and thereby make their own status within it central rather than marginal. It is a mistake, Hoffman notes, to see such Jews (as their own Jewish critics saw them) as kowtowing to Christians; rather, they were expressing ambivalence toward Christianity and toward their own acculturation.

Hoffman uses an ambitious array of sources, written in five languages over a century and a half. This breadth allows him to come to broad conclusions, but at times makes him elide differences. His argument that "the Jewish reclamation of Jesus reflects a more aggressive approach by Jews to participating in Western thought and culture than is usually acknowledged" (p. 5) is easier to accept in relation to those cultural products that were accessible to non-Jews—the German writings of the *maskilim* and the visual arts—than in relation to texts in the exclusively Jewish languages of Hebrew and Yiddish. But the aggression inherent in these works was directed as much inward as outward. And the artists knew it: Hoffman quotes the Russian Jewish sculptor Mark Antokolsky creating a Jesus in sidecurls and skullcap and imagining "how Jews and Christians alike will rise up against me. Jews will probably say, 'How is it that he made Christ?' And Christians will say, 'What kind of Christ did he make?' " (p. 212).

Although he carefully delineates variations in Jewish depictions of Jesus, Hoffman does not always admit that non-Jews, too, disagreed about his significance. Not only did Protestant, Catholic, and Eastern Orthodox Christs vary, but Jesus could take on meanings beyond those in the Gospels. Take Lamed Shapiro's horrifying Yiddish pogrom story, "The Cross," about a Jewish Socialist Revolutionary, branded with a cross, who turns from a would-be martyr into a rapist and murderer: it exemplifies not only the modernist Jewish appropriation of Jesus but also the grotesque use of Christian imagery typical of Russian fiction about Socialist Revolutionary terrorism in the years immediately after the 1905–1907 revolution. (Those interested in Shapiro's remarkable stories should see a new edition, *The Cross and Other Jewish Stories*, edited by Leah Garrett and published by Yale University Press in 2007.)

These complaints, though, are minor when set against Hoffman's achievement. His book offers a penetrating analysis of a controversial and still relevant phenomenon. It is a crucial new contribution to the study of modern Jewish history, Yiddish literature, and Jewish-Christian relations.

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STEVEN E. ASCHHEIM. *Beyond the Border: The German-Jewish Legacy Abroad*. Princeton, N.J.: Princeton University Press. 2007. Pp. xi, 194. $27.95.

Steven E. Aschheim's new volume is an essayistic treatment of the fortunes of German-Jewish intellectual and cultural life outside Germany and across an astonishing range of time. Bearing—for better more than for worse—clear signs of their origins as a series of lectures, the book's chapters deal with three successive generational moments in turn. The first is a study of German-Jewish émigrés in the binationalist movement in Palestine and Israel, from the interwar years to the early postwar era. The next shifts the scene to America, where a subsequent cohort of German Jews—most of whom, like Peter Gay and Fritz Stern, emigrated as young men—became some of the country's leading historians. A last chapter takes up the problem of why, even if we are not all German Jews (as French students in 1968 famously claimed), nevertheless German-Jewish intellectuals continue to be so central to our intellectual canon. In this way, Aschheim's study can culminate in the present, in reflections on why it is that the global academic public so often sees the world through German-Jewish lenses.

Aschheim's talent and inclination tend to the synthetic overview, even while he has a gift for the choice of example. His vivid presentation of the lives and times of his protagonists demonstrates his full mastery of the sources, but throughout Aschheim's stress falls on often newly available and freshly mined diaries and letters that allow him to knit his dramatis personae into a network. "One wonders," he remarks at one point, "whether or not those who have separately canonized these thinkers have any idea of the intellectually engaged and often charged personal relations that obtained between them" (p. 96). Correspondingly, the texts and arguments these figures offered in sometimes dense form do not drive the interpretation, their individuality and frequent difficulty forbidding the kind of synoptic *tour d'horizon* Aschheim has attempted here.

As befits a series of soundings, the book is narrowly selective in the topics it covers, even as it casts much

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new light on previously unexplored dimensions of *Exilforschung*. Even so, the breadth of the topics and figures included permits only the broadest of unifying theses: his protagonists, Aschheim says in his introduction (there is no conclusion), "acted as legatees and incarnations of German Jewry's cultural proclivities abroad, exemplified its strengths, biases, weaknesses, demonstrated again its accomplishments and conflicts, and analyzed, overcame, or replicated its blindspots" (pp. 4–5). Nevertheless, there are two main claims—one potentially in tension with the other—that do the most work.

In a supplement to his mentor George L. Mosse's *German Jews beyond Judaism* (1985), Aschheim previously argued that the ideal of liberal self-cultivation (*Bildung*) that had made this subculture so specific from the beginning ran out in the early twentieth century. But the same stale ideals prematurely rejected by German-Jewish youth, Aschheim now finds, were later rediscovered as invigorating fresh air. The German-Jewish binationalism of Shmuel Hugo Bergman, Martin Buber, Arthur Ruppin, Gershom Scholem, and others reveals that its inventors "retained" the "cultivating ethical and spiritual dimensions" of *Bildung* and "rendered its humanist and cultural impulses central to both their radicalism and their specific brand of nationalism" (p. 14). Émigré historians of the generation of Gay and Stern, whose sensibilities were in any event very different than those of Weimar radical intellectuals, carried German-Jewish humanism with them, making its Enlightenment values, in different versions, their "personal credo" (p. 63). In contrast, when he comes to the difficult problem of the "iconicity" of German-Jewish thinkers in contemporary intellectual life, Aschheim settles on the argument that it is their uncompromising dissidence that makes them exciting: that they were "contemptuous of the bourgeois present, [and] wary of easy liberal duplicity" is the likely cause of their appeal (p. 102). Here, "decent, humane liberalism" is presented as an external force, leaving the question of how German Jews could have incorporated it to their politics in the Middle East, and championed it in America, where their contemporary enthusiasts must seek it somewhere else, to balance to a potentially dangerous radicalism (p. 117).

We are left with a book brimming with glittering local insights, authoritative syntheses of older and newer scholarship (generously referenced in footnotes that occupy almost half of the book), and fascinating juxtapositions, such as Aschheim's contrast of the émigré historians with the concurrent German tradition of social history. As for what the German-Jewish legacy ultimately meant "beyond the border," readers will find in this book a variety of answers that do not always fit together. But it would seem that this rich and provocative but self-contradictory and inconclusive result perfectly matches the nature of the ongoing bequest German Jews have made to the world, in spite of the passing of their era and the dissolution (and destruction) of their historical conditions of possibility.

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ASIA

Cynthia J. Brokaw. *Commerce in Culture: The Sibao Book Trade in the Qing and Republican Periods*. (Harvard East Asian Monographs, number 280.) Cambridge, Mass.: Harvard University Press. 2007. Pp. xxiii, 673. $44.95.

In this magisterial study, Cynthia J. Brokaw explores the publishing and book selling enterprises of two extensively intermarried families in Sibao, an area she aptly describes as the "periphery of a periphery" (p. 77), an isolated and generally impoverished area of Fujian province in southeastern China. These people were Hakkas, not Han Chinese, immigrants from farther north who sometimes fought for land and often turned to sojourning commerce and handicrafts in addition to marginal farming. The Ma and Zou families began publishing soon after 1600; they stopped printing in the 1940s and their last bookstore closed only in the 1950s. They turned the plentiful labor and cheap local resources of hardwoods, bamboo, and pine into the printing blocks, paper, and ink for xylography, a simple technology that could produce books with minimal investment and only a handful of skilled craftspeople. At their commercial height during the eighteenth century Sibao printers supplied books to ten provinces, although they focused on the parts of Jiangxi and Guangdong closest to their production centers in mountainous western Fujian. Some scholars identify Sibao as one of the four major Qing period publishing areas.

Sibao printers sold no fine editions or scholarly books. They limited their production to guaranteed best sellers for the poor and poorly educated, catering to their desire for educational and cultural advancement and their practical needs: in addition to textbooks and references for civil service examinations, they produced medical books and guides for writing legal contracts. They sold cheap, small-format editions along the waterways and postal roads in the hinterland, peddling them at market towns and supplying their own shops in administrative centers. With good business sense, generation after generation of the two families brought culture to people—many of them other Hakkas—in the small towns of a vast region, surviving even the drastic decline in their textbook trade when the imperial civil service system ended in 1905. Only the introduction of even cheaper Shanghai lithograph and letter press editions forced most Sibao printers out of business.

Brokaw's sources include local histories, rare business records (account books, bills of sale, pawn records), scarce genealogies and property division agreements, and an exhaustive list of other primary and secondary materials. Her study is enriched by over a hundred interviews with descendants of the publishing

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families, some of whom worked in the trade as children early in the Republican period. It is illustrated with eighty-five photographs of Sibao imprints as well as maps, tables, and genealogical charts.

Brokaw's book consists of two substantial and carefully integrated studies. The first analyzes Sibao printing from geographical, social, economic, and political perspectives. Particularly significant contributions include outlining the ways that lineage organizations facilitated commercial ties and maximized efficient production, balancing the conservative values of the elite with the demands of extended business enterprises. Chapters here clarify economic history at both microcosmic and macrocosmic levels, from patterns of business decisions in printing households to the role of handicrafts in lineage economy. Brokaw's scrutiny extends from the carrying cases in which books were transported through the organization of printing households and their long-term financial strategies to the financial instruments and the system of banking institutions that supported long-distance transactions. She also traces the trade's annual business cycles.

Her second section examines their imprints: educational works, guides to good manners, health, and fortune, fiction, and entertainment. For each she outlines their contents and assesses their quality and cultural value; for many she includes the wholesale price as well. The volume concludes with overviews of Qing period "popular textual culture" and an impressively sophisticated evaluation of the social and geographical extent of literacy during the final centuries of imperial rule. Appendixes outline transport routes in the region, summarize available data on the value of printing blocks, provide genealogical tables for Ma and Zou publishers, list the names of their firms, site their various distribution routes and bookshops outside the region, and then list the names of all known Sibao imprints. Some of this data is also provided online for the convenience of others who wish to build upon Brokaw's research. The volume contains photos of buildings, equipment, and materials for block printing still to be found in the area.

This is a magnificent achievement in both the quantity of rare information Brokaw assembles and the quality of the insights she derives from this material. It will inspire book and cultural—as well as economic and social—historians of China to rethink earlier assumptions; it is a goldmine for the comparative study of literacy and cultural change in early modern societies as well. Its diverse contents and clear organization will allow reading—and assigning to students—individual sections or even separate chapters; every portion of this thoughtful study will stimulate new understandings and further research.

ROBERT E. HEGEL
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ZHANG ZHEN. *An Amorous History of the Silver Screen: Shanghai Cinema, 1896–1937.* (Cinema and Modernity.) Chicago: University of Chicago Press. 2005. Pp. xxxiii, 488. $30.00.

Film is a national idiom, argued George Amberg, then head of the New York University Department of Cinema Studies, long before the current interest in cultural studies. Zhang Zhen draws on this function in her study, finding a suitable metaphor for the rise of Chinese film in *Yinmu yanshi* (1931).

For years mainland Chinese film was ignored by the West. With the release of *Yellow Earth* (*Huang Tudi*, 1984) and the rise of the Fifth Generation filmmakers, however, Western academics flocked to the subject, treating Chinese film as their private domain. Finally we are beginning to see definitive writings by Chinese scholars, and this book is among the very best.

Rather than provide the usual litany of film titles associated with film histories, Zhang gives us an intensive and revealing analysis first of vernacular modernism as located in China, and specifically in Shanghai culture, from 1896, when film first arrived in China, to 1937, the year of the Japanese invasion. She notes the "capacity of cinema for serving as both a reflection of and an antidote to the stressful and alienating conditions of modern life" (p. 1). In the major section of her study, divided into several chapters, Zhang engages the specific issues surrounding what she terms "competing moderns." It is a perceptive strategy that in the process introduces a number of Chinese films hitherto known only to a few English speakers.

To prepare her readers for the materials to follow, especially her analysis of *Lustrous Pearls* (*Ye mingzhu*, 1927), Zhang ends chapter five, "Screenwriting, Trick Photography and Melodramatic Retribution," with an exceptionally fine analysis of "cinema as modern folk tale" (p. 190). Relatively brief, this essay draws on Vladimir Propp's examination of Russian folklore to link Chinese filmmakers and their films with the larger film community. Zhang's defense of her position is extensive and precise, ranging from a discussion of screenwriting and trick photography to an exceptionally fine treatment of the "modern girl" in terms of "hard" (*yingxing dianying*) and "soft" (*ruanxing dianying*) films in which she finds the left-right political debate over what constitutes the cinematic centered on the body of the modern girl and her destiny in urban society. This chapter and the chapter on the martial arts film, which precedes it, provide the most telling and significant elements of Zhang's book and should be required reading of anyone seriously interested in Chinese film.

If Zhang's study is exemplary, pointing the way to subsequent studies of equal intensity and precision, her prose, readable as it may be, merits further refinement. This is, of course, a relatively minor issue in relationship to the materials presented. Yet while I appreciate that the book is the rendition for the public of a scholarly dissertation, the requirements of that form tend to interrupt the quality of the presentation and might well

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affect its reception. Read this book and decide for yourself.

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JOSHUA GOLDSTEIN. *Drama Kings: Players and Publics in the Recreation of Peking Opera, 1870–1937.* Berkeley and Los Angeles: University of California Press. 2007. Pp. xi, 371. $49.95.

A tremendous amount has been written about Peking opera, in both Chinese and English. Much of this work, unfortunately, suffers from a tendency to catalogue information on the subject in overly reverential or appreciative terms. In contrast, Joshua Goldstein's book offers a richly detailed portrait of Peking opera that reaches far beyond earlier studies of appreciation and connoisseurship. With supple analysis and elegant prose, the study provides a critical history that illuminates the institutional, economic, and epistemological conditions of colonial modernity that gave rise to Peking opera in the nineteenth century and then mediated the process by which Peking opera became the performed embodiment of national culture in the twentieth century.

While careful to attend to the convoluted path taken by Peking opera, Goldstein nonetheless presents us with a narrative of profound discontinuity between nineteenth and twentieth-century China. Inspired by Michel Foucault and T. J. Mitchell, the author excavates a range of technologies, what he calls "everyday epistemologies," that underwent fundamental change to shape the historical development of Peking opera. We learn, for example, about the institutional shift from Peking opera's early rotational system of family-based troupes, which depended upon elite and court patronage, to far more commercially oriented, "star"-based troupes that needed to navigate the new world of mass media to gain fan support and success in the modern period. Innovative analysis of the social and physical space of theaters demonstrates how the "everyday technologies" of these spaces were linked to the cosmological ordering of the social hierarchies of the Qing empire and, in the following Republican era, to the ideals of citizenship and discipline of the modern regime. The drastic reconfiguration of gender politics during this transitional period and its impact on the popularity of opera stars is also explored. The late nineteenth century was the era of the *laosheng*, the older patriarchal character of operas, whereas the Republican era was epitomized by *dan* figures, female characters played by male actors, who, in the case of famous *dan* actors such as Mei Lanfang, could become national icons. These shifts in gendered identities were inextricably bound up with even more fundamental epistemological shifts. In a lucid discussion of difficult May Fourth–era debates on aesthetics and drama, Goldstein identifies a transformation from what he calls the "presentational" epistemological regime of the Qing empire (a regime of performance where social status is reproduced constantly through ritual, dress, and social space) to the modern episteme of "representationalism" during the new Republic, where a new social order disciplines its citizens and demarcates a fundamental split between representation and social reality.

As with many a fine study there are some imperfections. Whereas the first part of the book delivers powerful analysis and wonderfully rich historical portraits, the second part, which describes how Peking opera fared in the Republican era, seems at points somewhat repetitive. Parts of the discussion, including the author's treatment of reform in the opera world, as well as the gender politics informing opera, are foreshadowed in earlier chapters and thus lose much of their analytical punch. One could also argue that Goldstein's interpretation is perhaps too faithful to a Foucauldian narrative of epochal break, despite occasional attempts at qualification. This emphasis on radical break may even have resulted in a more romanticized treatment of nineteenth-century Peking opera and an overly negative portrayal of the disciplinary function of opera in the Republican period. In chapter two, Goldstein's portrait of the twentieth-century playhouse is strangely devoid of the sense of how historical subjects might have subverted or maneuvered the new playhouse space, particularly in contrast to his vivid and colorful description of how various agents of the late Qing teahouse—officials, actors, ordinary audience members, towel tossers—navigated and used that earlier space for their own purposes. While Goldstein remedies this contrast somewhat in chapter six, where he discusses the limits of reform and the persistence of "pesky table tenders," questions still remain: were twentieth-century middle-class consumers of opera as passive as his depiction might lead a reader to believe? Might those who appear to have "succumbed" to the new disciplining intentions of the playhouse still have shown agency in their attempts to demarcate themselves socially from the crass and crude mass consumers, even if it meant adopting new conventions of discipline?

Although one might wish for a more critical stance regarding the theoretical frameworks behind this study,it is nonetheless an example of the best kind of cultural history being written today. It delves deeply into the world of Peking opera even while moving far beyond the specific topic by vividly linking developments in opera with much larger contexts. Like some of Peking opera's most famous *dan* performers, Goldstein's bookpresents itself in an exquisitely elegant and seemingly effortless manner yet still exhibits tremendous virtuosity, subtle mastery, and impressive technique.

EUGENIA LEAN
Columbia University

EUGENIA LEAN. *Public Passions: The Trial of Shi Jianqiao and the Rise of Popular Sympathy in Republican China.* Berkeley and Los Angeles: University of California Press. 2007. Pp. xiv, 290. $45.00.

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This book is constructed around the story of Shi Jianqiao, who in 1935 assassinated Chinese warlord Sun Chuanfang to avenge her father's death at his hands ten years earlier. Under such lurid headlines as "Bloodbath in the Lay Buddhist Society," the case of the female assassin was a media sensation, through which, Eugenia Lean argues, Chinese society debated the proper place of traditionally valued forms of familial sentiment in a state now governed by a modern legal system. The natural drama of the case makes the book an engrossing read, but it is also an excellent survey of Chinese urban culture in the 1930s.

Lean locates her work as a response to the debate on whether or not late nineteenth or early twentieth-century China had anything that might be labeled a civil society in the European sense. This debate ran aground several years ago over difficulties in using definitions drawn from European history to describe Chinese society. Lean, however, takes it in a new and far more promising direction. She does this by comparing 1930s China not to *ancien régime* France but, much more plausibly, to the developing mass societies in Europe and America at the time. What she finds is political debate but conducted in very different terms from that suggested by Jürgen Habermas and with very different implications. This is the world of the mass media; of politics as scandal, sensation, and entertainment; of popular political participation that is active indeed but focused around emotional involvement in stories told by the popular press rather than rational debate among bourgeois men. Lean makes us look again at the new, and conflicting, ways in which Chinese in the twentieth century were invited to participate in politics.

Popular sentiment and its role in government are at the center of this book, and Lean's analysis of the debates that swirled around the dramatic assassination shows that this is precisely the issue on which they focus, for Shi Jianqiao presented herself to the public as a woman driven to murder by her love and respect for her deceased father. Such filial piety had been highly valued by the Chinese state for many centuries, and one of the most fascinating sections of Lean's book describes the use made by Shi Jianqiao's lawyers of classical texts such as the *Rites of Zhou* and *Gongyang Commentary* to argue for leniency on the ancient grounds that a son had the right to avenge a father who was wrongfully killed, and that a criminal who surrendered voluntarily to the authorities was displaying repentance and should therefore receive lenient treatment. Lawyers for the family of the assassinated warlord, on the other hand, argued for the necessity of applying the modern legal code, under which murder should be punished whatever the motive. For these lawyers and for many members of the elite, including left-leaning intellectuals, who supported them, Shi Jianqiao's emotions were out of date and her violent action recalled the chaos of the civil wars of the 1920s. The arguments in favor of applying the legal code prevailed in the courts, but the state ultimately intervened to pardon Shi, thus shifting the balance away from the courts and back toward the high value the public placed on acts of traditional filial piety.

Lean suggests that when the government made this decision to pardon Shi Jianqiao it was driven by popular feeling. She describes this feeling as "public sympathy" and argues that its rise was an important feature of the early twentieth century. The most interesting research in the book looks at the ways in which this public sympathy was shaped by the mass media and especially by fictional and dramatic adaptations of Shi Jianqiao's story that used the traditional genre of stories of knight-errantry to depict Shi as a heroine who deserved the reader's support in the face of a weak and potentially hostile state.

Despite its apparently narrow topic, this is an exceptionally wide-ranging book and one bursting with ideas—far more than it is possible to treat in one review. Its strength is in the breadth and sophistication of its discussion of 1930s ideas and culture: newspapers, fiction, drama, the legal system, militarism and violence, the state, and the relationship between gender and modernity are all interestingly discussed. Of course, the 1930s was also a time of industrialization, growing Japanese imperialism, and Nationalist Party state-building, something that it would be surprisingly easy for the reader of this volume to forget. These other subjects have already been well studied, however, and Lean's contribution of discussing the cultural history of the period is not only successful but also valuable.

HENRIETTA HARRISON
Harvard University

TAKII KAZUHIRO. *The Meiji Constitution: The Japanese Experience of the West and the Shaping of the Modern State*. Translated by DAVID NOBLE. Tokyo: International House of Japan. 2007. Pp. xix, 196.

This translation of Takii Kazuhiro's book, which appeared in Japanese in 2003, is one in a series funded by the LTCB International Library Trust via the International House of Japanto address the "shortage of foreign-language translations of Japanese books about Japanese subjects." No one can doubt that Japanese scholars are poorly served by the current situation, in which very few monographs find their way into English (or other European languages). The other major translation effort—the Library of Japan that started with the U. S. Cultural Commission and continues under the leadership of J. Thomas Rimer, with support from the Japan Foundation—focuses not on current scholarship but on autobiographies and other cultural monuments. Thus far, the LTCB series seems weighted in favor of economics and current events, but it includes as well books on kabuki, on the Tokyo war crimes trial, and this volume.

Takii's goal was to write a book for "the general reader," "treating the Meiji Constitution in terms of the process of the reception and adaptation of Western civilization in Meiji Japan" (p. 146). In the preface (p. vii), he speaks of making "possible a dialogue with the re-

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search in constitutional history being done in other countries."Hence this volume is a work of popularization in both its Japanese and English versions. It leans heavily on the research of other specialists.

This slim volume (ten pages of introduction, 143 pages of text) includes three chapters and conclusion; the chapters deal with the Iwakura Embassy (1871–1872), Itō Hirobumi's research in Europe (1882–1883), and Yamagata Aritomo's trip to Europe (1889); the conclusion is "The Meiji Constitution Seen from Abroad."Although far from parallel, the three trips provide a useful frame onto which Takii pins various aspects of Japan's attempt to learn from European and American constitutional thinking. Takii's own argument discounts textual analysis of the Meiji Constitution in favor of treating what he calls Japan's "actual constitution" (p. 96). He grants the constitution "unifying and symbolic roles" (p. 97), but he devotes most of his concern to the "viable national structure" (p. 95), the "fundamental structure of the nation" (p. 124) of which the constitution is only part. Included in this larger structure are the ancillary institutions that Itō Hirobumi revised in the run-up to the promulgation of the Meiji Constitution (1889): Privy Council, Imperial Household, and both houses of the Diet. Takii focuses on the role of various European scholars, most notably Lorenz von Stein in Vienna and Rudolf von Gneist in Berlin, who served as resources for the many Japanese bureaucrats who traveled to Europe in search of guidance. The search for guidance is as much the story as the documents that resulted. Takii's conclusion underlines the importance of European and American reaction to the Meiji Constitution: he cites comments by Albert Venn Dicey, Herbert Spencer, William Reynell Anson, and Henry Sidgwick in Great Britain, André Lebon in France, Stein in Austria, and Oliver Wendell Holmes in the United States.

Takii's emphasis on the "fundamental structure of the nation" calls to mind the term *kokutai*—literally, state form—that bedeviled Japanese political thinking throughout the era leading to World War II. In the reactionary version, *kokutai* transcended *seitai*, political form, and whereas Japan's *seitai* might be comparable with the structures of other states, its *kokutai* was unique and hence incomparable. But the word *kokutai* appears here only once, in Takii's conclusion (p. 137). Particularly for the purposes of comparative study by non-Japan specialists, that omission seems unfortunate.

The translation by David Noble is trustworthy and smooth and includes a useful appendix not found in the original Japanese version: the text of the Meiji Constitution (in Itō Miyoji's translation). The production values are very high, although the illustrations—particularly two letters handwritten in English by Itō (pp. 86, 122)—might profitably be twice as large.

Richard H. Minear
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Gregory Clancey. *Earthquake Nation: The Cultural Politics of Japanese Seismicity, 1868–1930.* Berkeley and Los Angeles: University of California Press. 2006. Pp. xiii, 331. $49.95.

No one who has visited Japan, where detailed reports of the latest seismic event appear almost daily on television, would be surprised at its characterization as an "earthquake nation." Yet, other than some recent work on the Great Kantō Earthquake of 1923, Western scholarship on Japan has hardly ever considered the place of natural disasters in that nation's transition to modernity in the late nineteenth and twentieth centuries. Gregory Clancey's innovative book addresses that oversight by arguing for the centrality of seismicity in the cultural and political development of Japan during the Meiji (1868–1912) and Taishō (1912–1926) periods.

The book is not a history of modern Japanese earthquakes as such but rather a study of "their emergence as objects of Meiji-period knowledge-making" (p. 4), specifically the role they played in the formation of architecture and seismology as professions in Japan. At the center of the book is the Great Nōbi Earthquake of 1891, "the strongest seismic event in modern Japanese history" (p. 113), which shook central Japan from Osaka to Tokyo and left thousands dead and tens of thousands homeless. What struck contemporary observers was that this cataclysm appeared to have brought down modern structures the Japanese had built on the advice of Western experts while leaving intact traditional, wooden buildings. Such perceptions prompted Japanese to question Western knowledge, making architecture one focus of the more general nativist reaction of the mid-Meiji years and leading to a unique Japanese invention—"Western carpentry"—that combined elements of foreign and native building traditions for the purpose of creating earthquake-resistant structures. At the same time, the modern science of seismology arose largely on Japanese soil, linking international recognition of Japan as an "earthquake nation" to its growing reputation as a world leader in this field of study. According to the author, however, in the aftermath of the 1923 catastrophe, that same characterization, tied to renewed discussion abroad of Japanese fragility, became a source of embarrassment, contributing to an ironic "submergence" of Japan as an earthquake nation (pp. 226–233).

The book provides fascinating detail on the key players in this drama, especially Englishmen Josiah Conder and John Milne, who arrived in Japan in 1876 to teach at the government's new College of Technology (Kōbudaigakkō), as well as the Japanese architects and scientists they trained. Conder, "the father of modern Japanese architecture," was determined to rebuild Japan in brick and stone, whereas Milne, "the founder of 'Anglo-Japanese seismology,'" began raising doubts about the purported seismic vulnerability of the country's traditional wooden architecture well before Nōbi (p. 6). Conder's Japanese successors would confront the post-Nōbi crisis in their profession initially by trying

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to make European masonry more earthquake resistant and ultimately by adopting ferro-concrete after the turn of the century. Meanwhile, Milne's students, who included the first professional seismologists in the world, would reinforce their mentor's claim for the tenacity of traditional Japanese buildings.

The book is impressive in its multidisciplinary approach, as the author draws on the insights of architecture, engineering, art history, and cultural studies alike. Especially engaging are Clancey's discussion of how the Conder-designed Rokumeikan—"a temple to cross-dressing of every kind" (p. 92), which the government used for extravagant masquerades—became a symbolic focus for the mid-Meiji backlash against Westernization, and his analysis of the way in which woodblock-print depictions of the destruction that the Nōbi disaster wreaked on the new Western-style landscape "nearly reversed colonizing tropes so common in Meiji discourse over the previous two decades" (p. 122). The author makes a compelling case for the cultural and political significance of the Nōbi earthquake, which has been almost totally absent from English-language histories of Japan. He notes, for example, that the earthquake, in drawing nationwide press coverage and spurring national mobilization for disaster relief led by the imperial family, prefigured the Sino-Japanese War of 1894–1895 "as a landmark 'nationalizing' event and a crucial moment in the popular representation of the Imperial institution" (p. 131).

Clancey's work challenges standard interpretations of Japan's emergence as a modern nation. We have numerous studies in English of the foreign employees (*oyatoi gaikokujin*) like Conder and Milne, whom the Meiji government hired by the thousands to assist in the official modernization drive, and of Meiji Japan's importation and adaptation of Western ideas, institutions, and technologies, but nothing that complicates the narrative of Japanese development after 1868 quite like this study does. The story of the Nōbi earthquake would seem to stand East and West on their heads or, as the author puts it, to represent "the mirror-image twin to classical histories of Japanese technology and industrialization" (p. 2). At the very least, the book will encourage students of modern Japan, as of other late developers, to take a far more critical view of such conventional and seemingly straightforward notions as "technology transfer."

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Steven D. Carter. *Householders: The Reizei Family in Japanese History*. (Harvard-Yenching Institute Monograph Series, number 61.) Cambridge, Mass.: Harvard University Press. 2007. Pp. xvi, 515. $55.00.

The invitation to review this book came as a pleasant surprise, because it treats a fascinating topic: the last of Japan's ancient court aristocrats remaining in Kyoto, where they have preserved family traditions for eight centuries—or many more, if one starts with their Fujiwara ancestors—down to the present day. The book does not disappoint. Historians of Japan, early or modern, will learn much from reading it. Yet I was also surprised that the *AHR* chose to review it, since the Reizei are a family of poets and Steven D. Carter a professor of literature. My impression is that historians of Japan, certainly in America, are apt to give only passing attention to classical poetry. Although Carter may not convince all such scholars to study poetry, he shows that they could learn much from studying a family of poets.

Despite the family's staid appearance today, the origins of the Reizei can be traced back to an unseemly dispute. Their lineage derives from one of early Japan's most admired poets, Fujiwara no Teika (1162–1241). In his grandsons' generation, his descendents quarreled over their inheritance. The mother of one, a literary figure herself, had possession of the family's manuscript collection, the key to its poetic legitimacy. She sued to have her son, born late in her husband's life, named heir. After losing her case in the ancient capital, she went to the shogun's court in Kamakura to try her luck. The case dragged on for years. A key defeat came in 1286, three years after her death, when her son was ordered to hand over the library to his elder half-brother. Instead, he turned to deceit, giving up a few texts of little value and keeping the best for himself. His branch of the family came to be known as the Reizei. It is revered today as Japan's last bastion of orthodox poetic tradition in large part because of its cultural capital, the precious manuscripts kept by illegitimate means.

The modern Reizei may view their history as one of remarkable continuity, but Carter shows how change played its part. Even if that may surprise no one, specific examples challenge familiar clichés concerning traditional elite culture, both civilian and military. Japanese aristocrats may have valued hereditary status, but noble birth guaranteed only the opportunity for success at court; actual success required the right combination of skill and luck. Although the modern Reizei remain in Kyoto, their ancestors occasionally chose to live elsewhere, sometimes because civil war drove them from their home, but other times simply because they could earn a better living, for example, in the warrior capital of Kamakura. That, in turn, reminds us that members of Japan's military elite continued to admire the civilian culture of the ancient court. The warriors of one sort or another who dominated Japan through virtually all eight centuries of Reizei history protected both the family and its poetic livelihood. This pattern persisted, albeit in very different form, even into the first half of the twentieth century. World War II, however, brought unwanted change. The last Reizei trained in its poetic tradition was drafted in 1944 and killed on the China front. After Japan's defeat, the occupation authorities abolished the estate rights that had provided the family's income. Eventually, like Gilbert and Sullivan's impecunious Duke of Plaza Toro, the family chose to incorporate itself and market its tradition to middle-class enthusiasts.

Since Carter covers a vast range of periods and top-

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ics, many historians will find material of interest in his book, but Carter is above all a literature specialist writing about a family of poets, and so poetry has a prominent place in this study. Japanese court poetry might be characterized as consisting of a few themes with endless variations. Carter does his best to help uninitiated readers recognize the stylistic distinctions and deviations from convention that may have startled poets trained in the tradition. He concludes his book with 100 poems, carefully explicated, by 100 poets who worked within the Reizei tradition. Members of the family predominate. Other authors range from the shogun Ashikaga Takauji (1305–1358) to a woman who kept a teahouse in Kyoto at the start of the eighteenth century. Readers unfamiliar with court poetry might want to read this appendix first to acquaint themselves with the Reizei poetic tradition. Some historians may find themselves skimming over Carter's discussions of poetry, which would be a loss, but far less of a loss than if they neglect this fascinating book altogether.

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FRANZISKA SERAPHIM. *War Memory and Social Politics in Japan, 1945–2005*. (Harvard East Asian Monographs, number 278.) Cambridge, Mass.: Harvard University Press. 2006. Pp. xv, 409. $49.95.

At the beginning of this carefully researched, richly informative survey of the politics of Japanese remembrance of World War II, Franziska Seraphim makes the astute observation that the Japanese government has served less as a leader than as a foil in the domestic political competition over war remembrance. Although individual and national war responsibility were always a part of Japanese discourse—no amnesia, to be sure—such questions were usually articulated by special interests contesting state support for, or infringement of, the prerogatives of war-bereaved citizens, Shinto shrines, and schools. Seraphim's analysis of this competition relies on an organizational and ideological history of five social interest groups from across the political spectrum. The strength of her approach is that it conveys a broad picture of the contest even as it exposes political interests behind the various ideological positions.

Moving more or less from the right to the left side of the political spectrum, Seraphim's first organization is the Association of Shinto Shrines. Shrine Shinto survived American occupation reforms separating church and state by asserting its own liberation from the control of the militarist state. At the same time, however, the shrine association bucked the trend of the times by asserting a continuity across the 1945 divide, embracing the emperor as the embodiment of the Japanese people's essential spiritual unity (and perhaps war innocence), and eventually working with the ruling conservative Liberal Democratic Party (LDP) to re-institutionalize imperial ritual as official national policy.

The Japan Association of War-bereaved Families (known as the Izokukai) coalesced among supporters of war widows and bereaved families whose military pensions had been eliminated as a demilitarization measure. Although politically and organizationally diverse at first, after pensions were reinstated in 1953, the Izokukai increasingly worked to valorize the war dead as national heroes (*eirei*) rather than war victims. This revanchist shift, which included lobbying the ruling conservative LDP for state support of the deceased's enshrinement at Yasukuni Shrine, was highly controversial even among Izokukai members. Much of the association's strength, however, resided in the ostensibly apolitical benefits membership provided, including pensions and connections to an established local social network of families.

The Japan-China Friendship Association has a diverse membership of businessmen, scholars, officials, and politicians with generally but not wholly progressive interests in promoting ties to China. It served as a liaison for repatriation and early trade and diplomatic relations and to this day continues to insist on official acknowledgment of Japanese war atrocities in China. Until the Cold War ended, the Friendship Association's educational influence was limited by public suspicion of communism, but since the 1980s public support for its agenda has increased substantially.

The leftist Japan Teachers' Union (JTU) arose as a true workers' union, aided by a collective sense of blame for war complicity and a desire "never to send our students to the battlefield again." In a fine chapter entitled "Forging Political Subjectivity," Seraphim traces the JTU's evolution as a vocal opponent of bureaucratic inroads in education and conservative efforts to reinstitute elements of the wartime imperial order. The JTU attributed the debacle of wartime Japanese complicity and victimization to systemic features of the imperial order. Both the JTU and the Izokukai understood the nation-state to be the proper locus of historical identity, but where the bereaved families' association posited an organically linked relationship characterized by mutual obligation, the teachers' union saw an antagonistic and oppressive relationship between citizen and state.

The pacifist Japan Memorial Society for the Students Killed in the War (generally known as the Wadatsumikai) coalesced around published writings of student-soldiers whose deaths symbolized the existential isolation of the intellectual in a society engaged in total war. Where the Shrine Shintoists validated an unchanging essentialist national community centered on the emperor, Wadatsumikai promoted universal humanist pacifism as the guiding principle for postwar Japan.

The middle chapters of Seraphim's book address how each of these prominent interest groups interacted with and adapted to the changes in domestic political culture from the 1950s to the 1980s, including generational transition and the rise of civic activism regarding Viet-

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nam and the environment. Readers of the *AHR* will be especially interested in the final section that analyzes how, beginning in the 1980s, the previously self-contained, interest-based domestic contest over war memory evolved in the spotlight of a global, ultimately post–Cold War, rights-based discourse of restitution. In her brief concluding chapter Seraphim contrasts Germany's experience to argue that Japan's public war memory is best understood as a fragmented domestic political culture of special interests.

The inability of the Japanese government to address the question of war responsibility in a totally forthright manner is a natural consequence of this fragmentation. In her analysis of the domestic entanglements encumbering Japanese public war memory, Seraphim has made a major contribution to our understanding of the political and cultural nuances of war remembrance in postwar Japan.

James J. Orr
Bucknell University

Eugene Y. Park. *Between Dreams and Reality: The Military Examination in Late Chosŏn Korea, 1600–1894.* (Harvard East Asian Monographs, number 281.) Cambridge, Mass.: Harvard University Press. 2007. Pp. xiv, 273. $39.95.

One of the more encouraging developments in the study of early modern and later Korea during the last two decades has been the increasing boldness of scholars both in South Korea (although not yet under the one-party regime of North Korea) and abroad to question the established frameworks and theories of Korean historical patterns.

A new generation of meticulous researchers has broken free of narrow, nationalism-inspired investigations and writings that used to stress "we too" pride in Korean accomplishments, either for validating top-down conventional narratives or for discovering sophisticated ideological wellsprings in episodes of popular resistance. It is not that such frameworks and theories were entirely groundless or contrived; they were especially relevant to the study of events from the late nineteenth century on, but they tended to overshadow or weaken any attempt to bring in more conceptual or interpretive nuances in accord with the often messy and inconvenient truths that littered the Korean sociological landscape. Equipped with access to richer primary source materials thanks to contemporary technology and better availability of both archival and private collections, and relying on the more profound, reality-based insights gained from contemporary sociology, new scholars are producing works that illuminate the Korean historical trajectory with greater credibility than the celebratory narratives of nationalist historians.

With his study of the military examination system during the years 1600–1894 of Korea's Chosŏn dynasty (1392–1910), Eugene Y. Park joins the ranks of those who are contributing to this new perspective. His main purpose is to show how Chosŏn society kept its balance between a stratification that in its Neo-Confucian ideology and often in law was as rigid as any and the pressures from those marginalized on the basis of birth, occupation, or residence by the power center of the capital city, Seoul. While the yangban aristocracy of the capital held the levers of civil and military power tightly, with the blessing of the throne, the aristocracy and the monarch were not above making periodic concessions—symbolic, psychological, and even material—to those from below who sought to join their privileged circle.

The latter might not, with the rarest of exceptions, be allowed into the select group in the capital, but some received trappings and benefits that would have made them feel psychologically proximate to it. The military examination system that was designed to recruit the equivalent of commissioned officers today was the less prestigious path to such marginal upward mobility (compared to the civil service system) and was the ruling elite's preferred choice for "inclusion" from below. Commoners with financial means, martial training or martial self-image, and literary ability, for example, built up what Pierre Bourdieu calls social capital by acquiring Neo-Confucian learning and mastery of family and social rituals, by displaying loyalty to the throne through service or cash donations, by practicing notable forms of filial piety within their clans, and by demonstrating other Confucian virtues in their communities. With that social capital they competed for examinations or bought degrees, ranks, and office titles—without always gaining actual high-level appointments. Certain tax and legal benefits sometimes accompanied such symbolic attainments and gave the recipient an aura of being close to the ruling elite if not part of its power structure.

In fact, the "elite" was itself stratified into multiple categories, and high status for some strata depended on regional locales and local esteem, as it did for the top echelons of the capital's upper-level bureaucracy. Status clearly was not a uniform concept but had to be interpreted in different sociogeographical contexts. To the system of checks and balances between the throne and the central bureaucracy that earlier studies by Edward Wagner and James Palais have amply documented, Park's book adds this broader national equilibrium—although it was always an uneasy one. This combination, despite sometimes violent challenges from below to corrupt, incompetent, and exploitative officials, kept the dynasty going, however unsteadily, until very late in the nineteenth century. Few among those who led an occasional mass rebellion had any grand vision or ideological agenda; most simply wanted to join the system as it was and rectify its abuses. By relief measures, punishments, and rewards, the throne and the central bureaucracy always managed to quell such rebellions. In this respect, Park reinforces the conclusion of Sun Joo Kim's study of the 1812 Hong Kyŏngnae Rebellion, *Marginality and Subversion in Korea* (2007).

Park's book (like Kim's) is not calculated to win

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friends among those who continue to see ideology as a major driving force in Korean history all the way back to the early decades of the nineteenth century, but it is generally very well grounded in finely honed arguments backed by both solid statistical analysis and apt anecdotal evidence. As such, it should be able to hold its ground firmly against critics, except perhaps on the issue of the Ch'oe Cheu–inspired uprisings of the early 1860s. Park rather casually dismisses the latter as "riots" without fully engaging with what some perceive to be refrains of egalitarianism running through them and what others see as a kind of "progressive" ideology if not movement.

When a book is heavy on sociological analysis rather than being a work of sustained storytelling, it is unlikely to appeal to all but the most dedicated readers. I am afraid Park's prose is on the dry side and often too complicated to be "easy on the eye," but that is true of most sociological histories; it may be the nature of the beast. Otherwise this is a highly commendable work of scholarship.

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THOMAS R. TRAUTMANN. *Languages and Nations: The Dravidian Proof in Colonial Madras*. Berkeley and Los Angeles: University of California Press. 2006. Pp. xv, 304. $49.95.

In the book under review, Thomas R. Trautmann writes that the development of the Indo-European idea presupposed the genealogy of nations in the Bible and applied it to languages; in other words, the idea of a family of languages and of genetic connections among them was not "naturally given and had to be fashioned from the idea of a genealogical tree of nations" (p. 214), a comparison that was very highly processed. It is this idea that the author seeks to pursue in a discussion of early nineteenth-century Madras in order to understand how both the idea and the proof of the Dravidian family of languages developed and were articulated. Trautmann believes that interest in the Dravidian proof worked itself out in colonial Madras because it was an "exceptionally privileged" (p. 230) site for the languages and nations project in its search for more "precise ideas of phonology and morphology" (p. 220). What India offered this essentially Orientalist project was a linguistic culture that contained "ideas of regular sound shifts and systematic etymological procedure that both Sanskrit and Tamil offered."

It is clear that this language-based project was a means of understanding the inner thoughts—the souls, if you will—of local people. Language learning analysis and comparison was a way to govern Indians and also to pacify them.

The substance of the book is contained in the chapters on the foundation and working out of the College of Fort St. George, an institution that was, in a sense, an instantiation of the ideas and work of Francis Whyte Ellis (d. 1819). Part of Trautmann's discussion is devoted to the ways in which Ellis and his circle both identified with but also separated themselves from the "Calcutta school" of Orientalism. Calcutta was, of course the site where William Jones, who announced the Indo-European language family in 1786, had worked. In effect, Calcutta became the site where the linguistic unity of India was articulated.

There were several institutions that were associated with the "Madras School." Aside from a Literary Society, the most important element was Fort St. George College. Ellis was able to found the college to teach Telugu and Tamil to the young employees of the Company, so as to enable them to work effectively at the local level almost everywhere in Madras Presidency.

Most of Trautmann's book is concerned with Ellis's other accomplishment: the articulation that Tamil, Telugu, and other languages of the south were not Indo-Aryan but were all part of the Dravidian linguistic family. Trautmann shows that Ellis's work was a part of what he calls the "Dravidian idea," which was accomplished by the work of Constantius Beschi, by the assistants of Colin Mackenzie, and by the report of the committee that set up the College of Fort. St. George, a quintessentially dialogic activity. Not only did the college enable junior Company employees to acquit themselves well in their work, but they were able to keep the population quiescent, a major goal of imperialism. We also know that a major effect of the college was the encouragement of persons of talent within the Indian population who published works of Telugu and Tamil scholarship. Some of these Indians, such as Mamdi Venkayya, a Komati merchant, went on to make signal contributions to Telugu lexicography.

Trautmann shows that, in essence, the Dravidian proof consisted in showing that the deshia words of Telugu were "traceable to roots found not in Sanskrit but in the South Indian languages generally" (p. 157). Ellis's "Dravidian proof" was included as an appendix to Alexander Duncan Campbell's *A Grammar of the Teloogoo Language* (1816), whose analysis of the deshia words in Telugu "is not the plain meaning of ancient authors" about Prakrit. Rather, Campbell was "really expounding a new idea which came about when the European and Indian traditions of analysis came together in British-Indian Madras" (p. 165). Trautmann therefore contends that the "Dravidian proof is an act of reinterpretation that validates the existing classification while revealing its hitherto hidden rationale."

Trautmann's work is full of erudition and insight about how the two traditions of Indian and European tradition came to fruition in the work of Ellis in colonial Madras. What Trautmann does not say is that Ellis is even more important for several other contributions to our understanding of south Indian and particularly Tamil culture. As I have shown in another context, Ellis is the person who documented the connections between the sengunthars and the study of Tamil, something that is reflected in the fact that C. N. Annadurai, the leader of the nativist Dravida Munneta Kazhagam in the

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1960s, was, in fact, a sengunthar. Ellis was also responsible for identifying and understanding the extent and variety of the Tamil country. He helped to establish what was originally a vernacular section of the Madras records, where all the documents were only in either Tamil or Telugu.

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TIM ALLENDER. *Ruling through Education: The Politics of Schooling in the Colonial Punjab*. (Asian Studies Association of Australia, South Asian Publications Series, number 14.) Elgin, Ill.: New Dawn Press. 2006. Pp. xiii, 358. $45.00.

Tim Allender's focus on British educational policy in Punjab is a shift from nationalist historiography on the Raj, and also from postmodern discourse on non-elite, subaltern resistance movements. At first glance, the author seems to evoke nostalgia for imperialist history through the use of colonial spellings for Indian terms, and in a reference to British subjugation of Ranjit Singh's "lawless successors" in "two bloody wars" before annexation in 1849 (p. 7). Yet, Allender's work is informed by nationalist and liberal Western scholarship, and it criticizes the Raj for not facilitating a dialogue between rulers and subjects, and for its failed educational policies. Allender calls his work a revision of subaltern history, which he dismisses summarily for having taken the focus away from critical studies of imperialism. The author reiterates that the initial British autocrats valued Punjab's rich farmlands so highly that they subsidized this province to make it an imperial showcase, thus ensuring Sikh loyalty during the 1857 Revolt. Punjab's privileged position was ensured by other devices such as lower revenue demands for the imperial exchequer until 1870, and the racial rhetoric contrasting Sikh military "masculinity" favorably to Bengal's "effeminacy." This was unusual, since Bengal was the capital, and provided a greater percentage of its revenues to the imperial budget (p. 224).

Allender makes a valid point that colonial policies were neither uniform nor preconceived, and that they evolved over time. He disagrees with scholars who suggest that education served as an administrative tool to control a recalcitrant subject population, although it eventually catered largely to the elite, some of whom later led the nationalist resistance. Allender reveals the theoretical biases of individual administrators and their disputes over language and curricular texts. These men often acted in response to changed circumstances, but good intentions were sometimes negated by the expanding state, and the exigencies of governing so complex a region as India. The author lucidly describes the idealistic policies of W. D. Arnold, Punjab's first Director of Public Instruction (DPI), and their overhaul after the 1857 Revolt by Lieutenant Governor Robert Montgomery, whose authoritarian approach favored centralization. Furthermore, the policy of decentralization in 1870 was intended to grant local autonomy to district officials, but instead it increased provincial authority. Thus, Arnold's democratic plan to educate the general populace through the system of Halkabandi village schools was overturned by a provincial system catering to the elite collaborators of the Raj. While the peasants paid the bulk of the revenue, wealthy landlords reaped the benefit. Allender argues that a failed educational promise became the fodder for nationalist discontent after 1874.

While Allender effectively demonstrates that British infighting caused early cracks in the Raj, his argument that these preceded Indian national identity may be somewhat overstated. Subaltern histories inform us of peasant and tribal revolts, and local army mutinies (Vellore in 1806) against the Raj, while the 1857 Revolt unified diverse groups across North India. The colonial era witnessed the crystallization of an embryonic national identity as Indians felt clearly distinct from the dominant British *other*. Moreover, regional literatures reveal that local ethnic affiliations were subsumed under, and coexisted with, an umbrella Indian identity. Moreover, while Allender acknowledges the works of Gail Minault and Nita Kumar on female education and missionaries in North India, this book would have been enriched by brief comparisons with works on Madras or Bombaysuch as S. A. Raman's *Getting Girls to School: Social Reform in the Tamil Districts, 1870–1930* (1996), Y. Vaikuntham's *Education and Social Change in South India: Andhra, 1880–1920* (1982), or Chitra Desai's *Girls' School Education and Social Change* (1976).

Allender's book is neatly conceptualized and organized into an introduction, eight chapters, a conclusion, four appendixes, maps, and illustrations. The introduction concerns the Anglo-Oriental debates, Thomas Babington Macaulay's Minute (1832) making English the *lingua franca* of India, and Woods Despatch (1854) with grants-in-aid for private schools. Chapters one and two describe Arnold's policy on vernacular medium Halkabandi schools, their proliferation through a one-percent cess; Montgomery's centralizing efforts and the abortive "female education movement." Chapters three and four examine the formation of a curriculum, translations from Western texts, and the medium of instruction (regional, Sanskrit or Persian, English); G. W. Leitner's tenure as head of the Orientalist College; the Anjuman literary society; and the changed direction from mass schooling to higher education for the elite. Chapters five and six treat the problem of inadequate texts, the Punjab University, and struggles between two factions—namely Leitner, his Indian supporters, and Viceroy Robert Bulwer Lytton against supporters of W. R. M. Holroyd, Director of Public Instruction (1868–1887). Chapters seven and eight focus on "decentralization" yet conformation to central standards; neglect of primary schools; and the liberal Hunter Educational Commission (1881–1882), which promoted primary and female schools for Hindus and Muslims. This section could have included a discussion of the educated Indians who assisted W. W. Hunter shortly be-

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fore the Indian National Congress was formed. Despite a few shortcomings, however, Allender's work adds to recent historiography on Punjab during the Raj.

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Rohan D'Souza. *Drowned and Dammed: Colonial Capitalism and Flood Control in Eastern India*. Oxford: Oxford University Press. 2006. Pp. xv, 264. $35.00.'

In 1946, at the dedication of the Hirakud Dam on the Mahanadi River in the eastern Indian province of Orissa, the governor proudly remarked that henceforth "flood, drought and famine will be banished" throughout the region. Yet, within the lifetime of Governor Hawthorne Lewis, over 100,000 peasants had been displaced from their homes. The cycles of drought and flooding continued unabated, and what benefits did accrue from the hydroelectric dam tended to be siphoned off to businesses and industries in places far distant from the peasantry for whom the project was ostensibly designed.

In this highly engaging and original study, Rohan D'Souza traces the events of a century of attempted flood control that resulted in such disastrous consequences. In the process, the author argues that each step of this journey must be seen in light of the invasive effect of capitalism on the deltaic environment of Orissa. "Colonial capitalism," he suggests, "in order to institute itself as a specific social form, deployed a set of hydraulic interventions that transformed the Orissa Delta from being a flood-dependent agrarian regime into a flood-vulnerable landscape" (pp. 215–216). From the moment that the East India Company introduced the Permanent Settlement in the greater Bengal Presidency, which privatized and gifted virtually all arable land to a new class of landlords known as zamindars, capitalism was the driving force behind flood control in eastern India.

D'Souza fortifies his thesis by emphasizing the actions of colonial engineers and their successors in three separate eras. The early sections of his book review initial imperial attempts to commodify water by building a series of embankments, insisting that zamindars pay for their upkeep, and charging the farmers a water usage fee. The results were contentious and entangled. Socially, landlords sued rather than pay for repairs, while the peasantry engaged in sabotage; as D'Souza confirms, "the embankment establishment ... [was] constantly harassed by the local populace" (p. 108). Furthermore, the engineers soon realized that the embankments, rather than protecting the land from flooding, in fact exacerbated its impact. The author suggests, however, that knowledgeable public works professionals were ignored by the Revenue Department, whose primary concern was profit.

The second period (detailed in the most powerful section of the book) concerns the brief privatization of the flood control/irrigation systems of East India in the 1860s. Swayed by the exaggerations of Sir Arthur Cotton, the most respected engineer in mid-nineteenth-century India, the government agreed to the formation of two private companies to construct and oversee water regulation in Madras and Orissa. The Orissa project in particular was a disaster from the beginning. Cotton had predicted that 2.5 million acres would be irrigated; in reality the East India Irrigation and Canal Company managed to supply water to less than ten percent of that acreage. In order to make a profit, the company resorted to forced levies and exorbitant charges; even so, both companies failed financially, and were taken over by the government within a few years.

By the 1940s, the new model for flood control and water distribution was the Tennessee Valley Authority, the Multi-Purpose River Valley Development (MPRVD) established during the Great Depression in the United States. The TVA administrators, along with the plan's admirers in India, saw the project as a grand experiment in social engineering, as well as a method of river management. MPRVDs, which included the Mahanadi Valley Scheme, would provide rural electricity, allow for the development of irrigation communities, and provide harmony in the hinterlands. In fact, as D'Souza suggests in his conclusion, the benefits accrued to the peasantry have been negligible. In the final analysis, he suggests, "the colonial watershed was not principally determined or shaped by drives for 'profligacy' or 'statemaking' but assembled by and derived from the working of colonial capitalism as a specific social form" (pp. 222–223).

South Asian environmental history is a young field of study, dating back only to the early 1980s. The historical study of public works and water control is younger still; with the exception of volume one of Elizabeth Whitcombe's *Agrarian Relations in Northern India: The United Provinces under British Rule, 1860–1900* (1972),a groundbreaking work on irrigation in North India, relatively few studies on water issues appeared until the late 1990s. More than thirty-five years after its publication, Whitcombe's work is recognized as a classic of environmental history. D'Souza's monograph, with its perceptive and persuasive argument that riparian control must be seen in light of capitalism's transformation of the meaning and purpose of nature, deserves to rest on the same shelf as Whitcombe's.

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Lisa Trivedi. *Clothing Gandhi's Nation: Homespun and Modern India*. Bloomington: Indiana University Press. 2007. Pp. xxvi, 205. $29.95.

Lisa Trivedi's book takes up the still flourishing topic of Indian national symbolism by exploring the production, consumption, and display of khadi or homespun cloth in India between the early 1920s and the 1940s. The book aims to challenge the largely Eurocentric slant of currently dominant theories of nationalism by

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focusing on consumption of the "visual" rather than emergent technologies such as print capitalism (p. xvii). The introduction suggests that the political importance of khadi lay not just in its economic significance in protest politics, but also as a medium for a set of powerful political symbols that resonated for audiences wider than India's political leaderships or urban middle classes.

Chapter one surveys the institutional histories of khadi production from the roots of Gandhian *swadeshi* during the agitations of 1905–1908 to the formation of the All-India Khaddar Board and All-India Spinners' Association. Trivedi provides rich and detailed analyses of both institutions, exploring the irony of how the khadi project experienced increasing success as it moved away from Gandhian principles (based on critiques of Western modernity) to gradually embrace middle-class consumer desires. In addition, the symbolic power of khadi from the late 1920s waxed as the "Gandhian" project to change consumer behavior waned with the development of regional and national distribution systems (pp. 28–31).

Chapters two and three set out the central arguments on the symbolic power of khadi. Chapter two, "Technologies of the Nation," explores the use of visual symbols in clothing and carefully examines how different manifestations of khadi drew on "tradition" while remaining essentially "modern" (pp. 39–41). Despite having downplayed the significance of print capitalism in her introduction, Trivedi goes on to examine detailed examples of printed khadi propaganda in their ambiguous representations of community and nation. Here as elsewhere in the book, the supposedly unifying political symbolism of khadi is taken largely at face value by the author: throughout, the success of national symbolism is implicitly judged by its ability to break down regional and communal particularity. However, the chapter expertly relates the ways in which khadi exhibitions drew different regions together into imagined national spaces, creating a new "material geography" (pp. 61–63).

Chapter three, "The Nation Clothed," contains probably the best research of the book, exploring the theme of how khadi colonized the Indian body. Particularly original here are the analyses of intersecting sexual and consumer desires (pp. 69–70), and studies of how khadi cut across community identifications around clothing and body adornments. This was a gendered process, as the embodied signifiers of cloth were more complicated and problematic for most Indian women than for men. The plain white style of khadi cloth was traditionally associated with low status and for many women could be publicly interpreted as a signifier of status rejection (p. 84). Even more interestingly, Trivedi looks at how the gendered symbolism of khadi served to contain male desire (pp. 84–85).

The final chapters (four and five) deal with time and space respectively. Chapter four, "The Rituals of Time," explores the invention of new "national" calendars in which khadi figures as a symbol, although arguably one of many other symbols in events where khadi was not necessarily central. This chapter and chapter five, "Inhabiting National Space," which looks at colonial reactions to the khadi topi and the flag ceremonies, are richly researched and presented in a useful and readable narrative. But we are left with the thought that such occasions perhaps tell us more about the nature of the colonial state than the detailed motives of the ceremony goers.

The strength of Trivedi's book lies in the quite originally crafted arguments about the contingent meanings of khadi as a national symbol. The conclusion rounds this up with an examination of state controls over the powerful symbols connected with the national flag. Trivedi has certainly provided an analytical and well-researched survey of "All-Indian" middle-class responses to khadi symbolism. However, the book promises to elucidate "the broad cultural currency of khadi" (p. xxiii), and the reader looking for grass-roots popular responses to homespun may be disappointed. This is perhaps the result of the author's assumption that "a cohesive Indian people became visible as more people dressed in khadi" (p. 94): that somehow a "single" Indian nation could ever be "visible." Resistance to the apparent homogenization of Gandhian anti-modernist Congress constructive nationalism is treated by examining elite critics, most prominently Jawaharlal Nehru and Sarojini Naidu (pp. 91–94). This raises the question about exactly how khadi was transfigured or adapted by Congress's intermittent grass-roots rural following. Despite these reservations,Trivedi's book represents the most prominent analytical study of the symbolism of khadi in this period, and in this sense it will be of great use to scholars and students of late colonial Indian nationalism.

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OCEANIA AND THE PACIFIC ISLANDS

WILLIAM A. R. RICHARDSON. *Was Australia Charted before 1606? The* Jave la Grande *Inscriptions*. Canberra: National Library of Australia. 2006. Pp. xii, 132. $27.23.

The focus of this book is not on which European nation discovered Australia but rather on the much narrower question of whether the Harleian or Dauphin map, done by French cartographers in the sixteenth century, is a pre-1606 charting of Australia. William A. R. Richardson's conclusion is an emphatic no, as it has been in many articles for over twenty years.

In proving the negative, language matters above all things. Richardson uses place names to show that a large peninsula on that map, called Jave la Grande, is not Australia but rather a combination of Java and Vietnam. In all fairness, the case for the linguistic origins of various place names is convincing, although not by any means unique to this author. They turn out to be French misunderstandings of the Portuguese and Javanese languages. In making his case for language over

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cartography, however, Richardson must perform some mind-boggling feats with the map. First off, he splits the Jave la Grande peninsula in half. The eastern portion then becomes Vietnam, but only after the image on the map is turned upside down and moved about ten degrees to the north. As for the western half, it becomes eastern Java by rotating the image from south-north to an east-west orientation.

Of course, sixteenth-century empirical discovery did not completely replace inaccurate ancient authority, so the maps of this era often represented a mixture of fact and myth. In this case Jave la Grande is shown as a peninsula extending northward from a mythical southern continent. A large number of researchers, including Kenneth McIntyre and Helen Wallis, have examined this combination of actual observation and revered authority on the French map and concluded it represents an early, imperfect view of Australia.

For Richardson, however, when deciding what is factual and what is not, language trumps cartography every time. To the naked eye, the eastern coast of Jave la Grande does bear a resemblance to the northeastern coast of Australia. In the same way the book's Figure 6, the largely inaccurate Bunting world map of 1581, shows a large island called India Meridionalis. It looks a lot like the western Australian coast and is located in roughly the right position. Richardson deems these similarities insufficient because the place names do not match the images. In chapter four, he states that if the place names used by sixteenth-century European cartographers do not contain some aboriginal-like words, then the land described could not be part of Australia.

This is a difficult argument to make, even under the best of circumstances. The overall haphazard structure of Richardson's book does not provide the best of circumstances. While the maps are first-rate reproductions, rather than giving them a unified numbering system and calling them illustrations, the author splits them into "plates" and "figures," each with its own set of numbers. Some so-called chapters are only a page or two long, less when plates and figures take up considerable space, as they often do. This fragmented approach leads to topics appearing multiple times rather than in one place. In the last half of this very short book, the reader must keep jumping back and forth to the relevant text and maps in order to follow the argument. The index is primitive, not always accurate, and provides little help.

As annoying as the structure is, it is not the worst aspect of the book. That dubious honor belongs to the author's application of standards to others that he cannot meet himself. To cite but one example, in chapter five he criticizes those who equate Australia with Jave la Grande on the grounds that the sixteenth-century Dauphin/Harleian map locates the peninsula twenty-four degrees too far west and five degrees too far north for it to be Australia. Since accurate longitudinal readings lay two centuries in the future and shipboard latitude could be tricky, that is a rather rigorous standard to apply. It is all the more so for someone who wants his readers to believe that the same site should be split in half, rotated, and moved over a thousand miles. That is applying a double standard with a vengeance.

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CANADA AND THE UNITED STATES

STERLING EVANS, editor. *The Borderlands of the American and Canadian Wests: Essays on Regional History of the Forty-Ninth Parallel*. Lincoln: University of Nebraska Press. 2006. Pp. xxxv, 386. $49.95.

In contrast to the close attention paid to the United States-Mexican border by politicians, policy makers, and scholars alike, the U. S.-Canadian border is a neglected topic. This collection, focusing on the Forty-ninth Parallel separating the American and Canadian Wests, aims to change that with essays by twenty-three scholars, twelve Canadians and eleven Americans. In common with most such collections, the contributions vary in quality, but most are well researched and several offer provocative commentary. The essays are grouped in six topical sections, each with a short editor's introduction and a topical bibliography (which might have been more usefully grouped at the back of the book). Those bibliographies evidence a significant body of historical work. Yet, as is frequently noted in this volume, the scholarly communities in Canada and the United States tend to operate within their own nationalist paradigms. It is fair to say that even border scholars are poorly informed about the work of their cross-border counterparts, although we Americans certainly win the booby prize for willful ignorance. As mid-twentieth-century Canadian historian J. Bartlett Brebner once quipped, "Americans are benevolently ignorant about Canada, while Canadians are malevolently well informed about the United States."

The essays narrate transnational relations, explore border cultures, and compare and contrast the two national Wests. They sprawl, although editor Sterling Evans works diligently to tie them together. A unifying thread is the problematic of border history. In their useful introduction, Thomas D. Isern and R. Bruce Shepard outline three approaches. The continental approach tells a story of environmental unities, natural and cultural regions, overriding international boundaries. The comparative approach sorts out the similarities and differences between the national Wests of Canada and the United States. The borderland approach focuses attention on the peoples, cultures, and landscapes physically adjacent to the border. This scheme also charts a history of Forty-ninth Parallel historiography over the past half century.

Consider Brebner, who wrote North American history from a "continentalist" perspective. In 1940, with immigration historian Marcus Lee Hansen, Brebner coauthored *The Mingling of the Canadian and American*

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Peoples, a forgotten classic detailing the migratory streams that moved back and forth across the international boundary in the nineteenth and early twentieth centuries. By the mid-twentieth century, a quarter of all Canadians by birth and parentage were living in the United States (as Brebner was). Unfortunately neither Evans nor his contributors pay much attention to the work and intellectual frameworks of continentalists like Brebner, or others such as Bernard DeVoto or Joseph Kinsey Howard, both of whom enjoyed large reading publics.

Continentalism, defined as a sensitivity to environmental and cultural unities that transcend the border, is evident in most of these essays. For a number, it provides the argument. The late cultural geographer Terry G. Jordan-Bychkov, in a persuasive piece written with characteristic wit, finds the international boundary had little effect on land use patterns. Cattle ranching north and south were both shaped by the stock-raising system developed in the early nineteenth-century American Midwest. "As far as cattle ranching is concerned," Jordan concludes, "the border does not matter" (p. 115). Evelyne Stitt Pickett contributes a fascinating historical investigation of the vast army of itinerant workers that tramped across the North American West during the long era that extended from the California Gold Rush to the end of World War I. These men (and women, Pickett shows) moved freely back and forth across the border to work in industries that were identical, north or south. Labor radicalism spread with the workers. The International Workers of the World (IWW) was truly international. The first local in Vancouver, Canada, was chartered in 1906, only a year after the organization's founding in Chicago. By 1911 there were some 50,000 members in western Canada, and about the same number in the western United States.

In the years following World War II, continentalism as an operating protocol of border history was challenged by resurgent Canadian historical nationalism and American consensus exceptionalism. The comparative approach offered comfort to both. The late Seymour Martin Lipset's *Continental Divide: The Values and Institutions of the United States and Canada* (1990), a book that summarized three decades of comparative work, best exemplifies the mature scholarly form. Lipset argues that the political and cultural distinctions between the two nations are rooted in different histories and traditions. Americans are individualistic, Canadians oriented to community; Americans are risk takers, Canadians exercise greater caution, reserve, and restraint; Americans see the world in terms of absolutes, Canadians are less moralistic. This kind of comparative approach, as useful as it is, can also feed stereotypes. In a cruder form, these contrasts are the fundament of American and Canadian vernacular understandings of each other.

Richer possibilities for a comparative approach are evident in Paul Sharp's *Whoop-Up Country: The Canadian-American West, 1865–1885* (1955), which is excerpted in this collection. Sharp explains the environmental unity of the northern plains and narrates the history of early commercial development in the years after the American Civil War that paid little or no heed to the border. Yet nationalism, Sharp argues, proved more powerful than environment in shaping the history. "Nature created a grassland region possessing a unity and integrity that its aboriginal inhabitants respected. The white man's politics rent it asunder." In his essay, Marian C. McKenna details that process with an enlightening comparison of the Canadian and American administration of justice on the two sides of the border in the late nineteenth century. In opposition to the standard argument that emphasizes the difference between unruly Americans and orderly Canadians, he argues that there was disorder and violence on both sides of the border and that these were "characteristic of frontier society" (p. 82). But what impresses most is precisely the distinction between the two colonizing systems: the messy and decentralized justice system in U. S. territory, the lawlessness and vigilantism, compared to the systematic approach of the Canadians, with the North-West Mounted Police combining law enforcement and judicial functions. In the end McKenna concludes that the American frontier "was freer, more open, untamed, unprotected, and therefore more prone to violence," while the Canadian frontier "was more rigidly controlled" (p. 104).

More recently, in an intellectual climate influenced by postcolonial theory, the trend has been toward a borderland approach. A key text is Beth LaDow's *The Medicine Line: Life and Death on a North American Borderland* (2001), which examines the history of natives, immigrants, settlers, and local and federal authorities in the Montana-Saskatchewan region during the half century from 1880 to 1930. Taking the local point of view, she argues for the shifting significance of the border in different periods: highly determinative during the years of state formation in the late nineteenth century, much less so in the early twentieth-century aftermath. Many of the essays here take the same approach. Lissa Wadewitz shows how the international boundary through Puget Sound offered fishermen and cannery workers—Canadian and American, Indian, Asian, and Anglo—greater opportunities while contributing to the devastating decline in the runs of the Pacific salmon. Gerhard J. Ens, author of *Homeland to Hinterland: The Changing Worlds of the Red River Métis in the Nineteenth Century* (1996), argues that the history of violence between the Métis of the northern plains and the Canadian confederacy "should give pause to those historians who still hold the belief that the Canadian West was settled peaceably" (p. 152). State-formation not only destroyed the world of people like the Sioux but that of the Métis as well. Yet the border also provided some refuge for both groups at the nadir of their struggle: for the northward fleeing Sioux after the Battle of the Little Big Horn, for southward retreating Métis following their defeat by the Mounties in 1885. Solid histories like

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these herald the emergence of an important new field of transnational North American history.

JOHN MACK FARAGHER
Yale University

RUSTY BITTERMANN. *Rural Protest on Prince Edward Island: From British Colonization to the Escheat Movement*. Toronto: University of Toronto Press. 2006. Pp. xii, 372. Cloth $65.00, paper $29.95.

In the past decade, studies of agrarian movements in the Atlantic world have taken a new turn, shifting away from rigid theoretical models toward empirically rich approaches that highlight the texture and contingency of farmer protest. This trend includes a conscious effort to compare forms of unrest across time and space. Rusty Bittermann does both exceptionally well, providing a detailed reconstruction of the Escheat movement on Prince Edward Island in the early nineteenth century that has direct ramifications for the study of rural movements elsewhere. Bittermann argues that Escheat's legacy came not in its failure to defeat landlordism in the 1830s; rather, it established a tradition of democratic activism that gradually insinuated itself on the structures of the state, despite the best efforts of its opponents, by the 1870s. In Bittermann's capable hands, agrarians emerge as advocates of a new, modern state that answered the needs of the people writ large.

As in other areas of North America that witnessed rural unrest in the eighteenth and nineteenth centuries, the issue of who owned, claimed, or controlled the land was central to the emergence and force of the Escheat movement. In an account with striking parallels to New York, Maine, Upper Canada, and other sites of farmer protest in this period, Bittermann reconstructs the history of crown grants on the island. In the wake of Britain's success in the Seven Years' War, the home government carved the land into great lots and distributed them to loyal placemen on both sides of the ocean. The new landlords agreed to settle a certain percentage of land within a short period and pay quitrents to the crown in exchange for the right to develop massive estates, some of which eventually topped 100,000 acres. These men rarely made an effort to either settle their estates or pay their quitrents, leaving legal title in doubt. Yet they demanded rents from tenants or squatters on their land and used the force of law to collect. By 1790, the first murmurings of discontent among farmers crystallized in calls for the creation of escheat courts to establish whether grants should be voided for non-compliance and sold to current occupants at a fair price.

In 1790, and through to the final political defeat of the Escheat movement in 1842, a constant battle between advocates of landed and industrial development on the one hand and agrarians on the other occupied center stage in the island's politics. Bittermann deftly reconstructs the creation of the landlord-developer coalition, which was founded on the notions that property rights were inviolable (that is, once one had at least a flimsy claim to them) and that men of property had the necessary spirit of enterprise to lift the island's fortunes through internal improvements and capital investment. This group enjoyed the backing of both royal governors and the Colonial Office. Hence, despite notable democratic mobilization on behalf of Escheat in the 1830s—including Escheaters winning the Assembly in 1838—land reform eluded islanders.

For their part, the Escheators rallied around an ideological message at odds with the dominant state-centered, elitist model of their adversaries. They argued that as producers, they had earned natural rights to property, political voice, and influence over the functioning of state institutions. That is, they challenged the status quo, arguing that the interests of the many should outweigh those of the few. The Escheat movement employed popular meetings, rent boycotts, political rallies, and violence on occasion, placing it squarely within the traditions of other agrarian or labor movements in Canada, the United States, and England at the time.

According to Bittermann, the Escheat movement suffered a fatal setback in 1842, when Lieutenant Governor Charles Fitzroy induced the Colonial Office to denounce Escheat as anti-developmental and to advise islanders to "turn their attention to improvement of resources, and the encouragement of the growth of Prince Edward Island" (p. 260). Therefore, despite widespread electoral support and lobbying by Escheat leader William Cooper on both sides of the Atlantic, the British government ruled out even modest land reform. To Bittermann, Fitzroy's victory was the last triumph of an older notion of state power and economic development: that is, powerand authority,not strict adherence to law (and certainly not to justice) determined the course of the island's development. This observation leads to Bittermann's salient point: though defeated, Escheators would prove to be the vanguard of a truly modern state. The movement established a democratic political culture that eventually led to mass political participation, state structures that met the needs of the larger portion of society, and mechanisms to neutralize the power of the select few. The island's population would ultimately see the end of landlordism in the 1870s as a result.

Bittermann's strength rests in his refusal to simplify the Escheat controversy as a class struggle. He demonstrates that Escheat alliances were fluid, with tenants, freeholders, entrepreneurs, landlords, and the crown flirting with or embracing the issue at various times. Bittermann's nuanced analysis, fluid writing, and exhaustive research make his book a major contribution to the history of agrarian movements in the Atlantic world.

THOMAS SUMMERHILL
Michigan State University

ERIC W. SAGER and PETER BASKERVILLE, editors. *Household Counts: Canadian Households and Families in 1901*. Buffalo, N.Y.: University of Toronto Press. 2007. Pp. xiii, 486. Cloth $85.00, paper $45.00.

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With this collection derived from its members' ongoing analysis of the 1901 Census of Canada, the interdisciplinary Canadian Families Project team continues to advance our understanding of the material basis, reproductive strategies, and internal dynamics of early twentieth-century Canadian families. The evidentiary base is daunting: data on 5,371,315 individuals was used to create a random sample of approximately 265,000 individuals and 51,000 dwellings. Yet the separate studies offer much more than a sense of "how many" and "how much," important though such information is. By means of creative methods and a careful layering of quantitative and qualitative evidence, the fourteen essays in this volume bring us closer to uncovering private lives of "ordinary" Canadians that are otherwise enticingly "off the record." They bring to light the salient trends and patterns that signify familial adaptation in an ongoing historical process to which families both respond and contribute. The contributors present a picture of the diversity of family types and family strategies within the context of a young nation coming to terms with the disruptive elements of "progress" that constituted modernization and the nation-building project.

The census categories necessarily define the nature and range of topics that can be considered, but the contributors have selected subjects that permit them to apply their individual expertise to the census material and also to draw upon external sources that inform and fill out that particular data. There is a fair mix of contextual and "national" analysis and more detailed, localized case studies. Chad Gaffield's piece on the problematic elements of census taking, the political and social concerns and objectives that underlie the construction of its categories, and the challenges presented by data as the basis of historical interpretation serves as an effective methodological "road map"; he also makes the often overlooked point that the census is situated in history as much as it is a historical document. Peter Gossage and Danielle Gauvreau's cross-province analysis of fertility and reproduction notes the cultural/religious influences at play as well as those of literacy and employment prospects. They conclude that the varying socioeconomic and cultural factors across provinces and regions, rural and urban centers, prohibit the making of one inclusive national model. Peter Baskerville's essay nicely complements and supplements this wider national analysis by exploring the "medical marketplace" in Victoria, British Columbia, for women giving birth. Lisa Dillon's exploration of the shifting meanings of age offers a valuable Canadian-American comparison as well as a necessary acknowledgement of how age and its boundaries are as much culturally defined and specific to time and place as they are biologically determined.

Also concerning the social construction of "norms," of particular relevance to a concept as idealized as "the family," Annalee Lepp discusses the conflicting relationship of familial ideologies with the realities of marital breakdown and its repercussions for household structures. Historical geographers Larry McCann, Ian Buck, and Ole Heggen contribute two compelling chapters on "Family Geographies" from national and urban perspectives. Their mapping of the data affords a visualization of such standard statistical categories as those of family size, nuclear, extended, and single-parent families, fertility rates and such less readily at hand information as that concerning members' contributions to the family economy. From historical sociology comes Gordon Darroch's important chapter, "Families, Fostering, and Flying the Coop: Lessons in Liberal Cultural Formation, 1871–1901." His comparative analysis of the 1871 and 1901 census data convincingly supports his contention that binding out, fostering, and boarding children and youth were common practice in 1871 and still very much so in 1901, thereby contesting standard interpretations that prolonged adolescent dependence was already a new-century development.

Bettina Bradbury's consideration of the condition of one-parent families in 1901 confirms that most children in such situations—as statistically common as in the twenty-first century, but more likely attributable to a father's death than to divorce—lived with their mothers. The stark gender imbalance in wage-earning opportunities and rates of pay that characterized the labor market assured them a life of poverty. Most intriguing is the attention that Bradbury pays to male-headed families, as, for example, was disproportionately the experience of Chinese immigrant children for whom the head tax kept mothers from joining the family in Canada. Eric W. Sager's solid analysis in "Inequality, Earnings, and the Canadian Working Class in 1901" leaves little question that the family cycle, profoundly shaped by the age and gender composition of individual families at specific moments in their history, was instrumental to its material well-being. As do most of the other essays cited here, Sager's work confirms that, ultimately, "among working-class families a large proportion were vulnerable to poverty, and vulnerability increased as one moved through the family cycle" (p. 359). Family historians will want to pursue the effects for individual family members, and how this influenced generational and sibling relations.

The contributions to this collection are grounded in thorough scholarship and are presented in an engaging manner. Aside from the opening essay by editors and project leaders Baskerville and Sager, however, there is little in the way of interconnection between and among the chapters, although their related themes beg for such connections to be made explicit. And there is also no denying that, despite the comparative analysis across censuses, regions and even national borders, the end result is a somewhat frozen-in-time portrayal of Canada and Canadian families as captured in the categories and numerical columns of the 1901 census. For all that, it is a rich and multitextured portrait that will support and inspire historical research on Canadian families and Canadian society across any number of disciplines for years to come.

Cynthia Comacchio
Wilfrid Laurier University

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XAVIER GÉLINAS. *La droite intellectuelle québécoise et la Révolution tranquille*. (Cultures québécoises.) Montreal: Les presses de l'Université Laval. 2007. Pp. xii, 486. $45.00.

Historians and social scientists have written extensively about Quebec's "Quiet Revolution," and with good reason. In the early 1950s Quebec was devoutly Catholic, politically and socially conservative, economically dominated by an anglophone elite, and seemingly resigned to its provincial status in what was then a highly centralized federation. By 1976 it was probably the most secular and socially liberal part of North America, the administrative capabilities of the provincial state and its intervention in the economy had grown beyond recognition, and its voters had elected a government pledged to transform Quebec from a province into a sovereign nation-state. Explaining what happened and why, and evaluating the significance of these developments, are tasks that understandably preoccupy a legion of Québécois and Canadian scholars, and will doubtless continue to do so for years to come.

As is probably true of the historiography of most revolutions, the winners of the Quiet Revolution have attracted much more attention than the losers, partly because much of the history and social analysis has been written by the winners themselves. In particular we have lacked until now a scholarly and comprehensive analysis of the ideas of the right-wing intellectuals who resisted the changes that were transforming Quebec in the 1960s. As a result it is often assumed that Quebec's intellectuals were unanimous in welcoming the secularism and social liberalism that followed the Quiet Revolution and even that the term "conservative intellectual," at least in Quebec, is an oxymoron.

Xavier Gélinas's sophisticated study goes a long way toward repairing this gap in the literature. Gélinas concentrates on a small group of conservative intellectuals—Lionel Groulx, Richard Arès, Esdras Minville, François-Albert Angers, Robert Rumilly, and Raymond Barbeau—during the pivotal decade from 1956 to 1966. The first part of the book outlines the political and social context of the time, the meaning of conservatism in Quebec and elsewhere, the various groups and periodical publications that propagated conservative ideas in Quebec, and the decline of their influence after 1962. The second and longer part of the book is a careful examination of the ideas of these Quebec conservatives concerning the three principal aspects of the Quiet Revolution: the growth of Quebec nationalism, the changing role of the state, and the declining influence of the Roman Catholic Church. A brief final chapter summarizes the author's conclusions.

The division of the book into two parts produces a certain amount of repetition, and the absence of an index, a regrettably common problem in Québécois books, is unfortunate in a scholarly work of this length. However, these minor defects are more than compensated for by the author's careful scholarship and thoughtful analysis, as well as the intrinsic interest of the story which he tells, a story that casts new light on the meaning and significance of the Quiet Revolution itself as well as on its critics.

Gélinas's research demonstrates some important facts about the conservative critics of the Quiet Revolution. Most of them were born either before 1910 or after 1925, unlike the Quiet Revolutionaries, who tended to be born between those dates. They were not as closely associated with Maurice Duplessis's Union Nationale as is sometimes thought and were often critical of it, although some of their periodicals and associations received financial support from Duplessis's government. Their ideas evolved in response to events. Although they had much in common they sometimes disagreed with one another on specific issues. Most of them supported some aspects of the Quiet Revolution, such as its nationalism and its use of the Quebec state as a counterweight to the power of the anglophone economic elite. However, they decisively parted company with the Quiet Revolution when its growing secularism and anticlericalism became apparent, and particularly when Jean Lesage's Liberal government sought to curtail the influence of Catholicism in Quebec's educational system. The right-wing intellectuals were also disturbed by the growing liberal tendency within the Catholic Church itself during and after Vatican II. They were torn between their obedience to the authority of the church and their belief that the church itself was no longer defending Catholic doctrine as they understood it.

Partly for this reason, partly because the secularizing trend proved irresistible, and partly because their nationalist ideas were being promoted more effectively by other leaders who seemed more in tune with the times, the conservative intellectuals rapidly lost influence after 1962. Yet Gélinas implies in his concluding chapter that their reputation may yet be rehabilitated as the legacy of the Quiet Revolution, and the moral and spiritual vacuum that has followed the collapse of clerical power in Quebec, are increasingly been questioned. The recent rise of the neo-conservative Action Democratique du Québec, which currently holds more seats in Quebec's National Assembly than the social democratic and sovereignist Parti Québécois, gives some credence to this conclusion.

GARTH STEVENSON
Brock University

PHILIP LEVY. *Fellow Travelers: Indians and Europeans Contesting the Early American Trail*. Gainesville: University Press of Florida. 2007. Pp. xii, 199. $59.95.

It seems a simple question: Where did Native people and European newcomers meet each other? The traditional answers have been equally simple: at trading places, in diplomatic exchanges, and on the battle grounds. Philip Levy's short but compelling book offers a fourth space: the trail. Drawing on a close and comprehensive reading of early American travel and exploration literature and sparked by his own experiences on

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the Appalachian Trail, Levy makes a persuasive case for trails as places where colonial travelers and Indian guides engaged in conversation, cooperation, and sometimes conflict. As he argues, "In sharing the rigors and burdens of the trail, and relying on one another in a variety of ways, Indians and Europeans entwined their fates as travelers" (p. 3).

Levy begins with a thoughtful reassessment of the sixteenth-century Spanish *entradas*, with special attention to Hernán de Soto's bloody track through the Southeast. While most accounts place de Soto at the center of the story, Levy shifts the focus to the ways Natives sought to shape the trail experience. Some fled from the invaders while others committed suicide rather than accept virtual slavery as guides and porters. Between these extremes Levy finds other ways Natives tried to control the direction and pace of the *entrada*. Slowing the march not only delayed de Soto but conserved Native strength. Armed resistance was an alternative but on the trail deception was perhaps a more available strategy. As Levy eloquently puts it, the Spanish were "led from place to place by coerced and terrified Natives themselves eagerly awaiting their chance to escape, ditch, or kill the men who held their chains" (p. 21).

The *entradas* remain a violent part of trail history but they do not provide the center for Levy's book. The sort of coercion practiced by the de Soto and Coronado expeditions slipped away in later years. Levy moves to examine other aspects of the trail experience. As Europeans sought guides for trail passages, what were the rewards each hoped to gain? Here Levy's close reading of the evidence pays off as he reveals both sides of a delicate negotiation. Europeans hoped the bargaining would provide reliable guides, careful translators, accurate geographic information, and a measure of travel security. For prospective guides the rewards were more complex but equally important. In a capitalist marketplace, guides expected payment in goods for their services. Perhaps more important, the role of guide offered status and prestige. As Levy explains, guides negotiated not only the journey passage but the passage before the journey began.

These negotiations at the beginning of the journey continued on the trail as fellow travelers argued about what might be called "the rules of the road." As Levy explains, "people brought to the trail differing assumptions about the best way to travel" (p. 63). Talking about the route and how to behave on the journey was a key way for Natives and colonials to understand each other. Those conversations were not just about following custom and practice on the trail. The travelers argued about the meaning of the landscape itself. What an English or Dutch traveler saw as a pile of stones or a blaze on the tree might have a very different meaning for a Native guide. As Levy observes, on the trail travelers moved through landscapes that blurred the boundaries between the physical world and the supernatural one. In what were often spirited debates, Indians and Europeans revealed their larger assumptions, understandings, and misunderstandings.

Perhaps the most intriguing chapter in this book is entitled "None but the Rattlesnakes!" Of all the changes and chances Native and Europeans encountered on the trail, none prompted more quick action and vivid writing than coming upon rattlers. How fellow travelers perceived these snakes and what to do with them revealed some of the deepest and most profound differences between Natives and newcomers. Natives saw snakes as powerful supernatural beings, often embodying the spirits of departed family or clan members. Humans and snakes could communicate with each other; snakes demanded both respect and appropriate gifts. Failing to deal properly with a snake could endanger the entire travel enterprise. The substantial European snake lore was mostly negative. Snakes were dangerous enemies to be feared and destroyed. As Levy concludes, "rattlesnake encounters show how the trail's unique conditions brought out the larger tensions often underlying the relationships between Indian and European travel companions" (p. 104).

This remarkable book opens a new space for understanding the complicated relationships between Natives and Europeans in early North America. It offers us a new cast of characters, a new stage, and a new way to read the script. Levy skillfully teases out Indian voices and meanings from European documents. Gracefully written and astutely argued, his book should have a place on the short shelf of works that can change our angle of vision about the early American past.

JAMES P. RONDA
University of Tulsa

KAREN ORDAHL KUPPERMAN. *The Jamestown Project*. Cambridge, Mass.: Belknap Press of Harvard University Press. 2007. Pp. viii, 380. $29.95.

The conquest of "Virginia" began in earnest in 1585. Thirty-seven years and several thousands of "lost" settlers later, Virginia as a colonization project was still teetering on the brink of extinction. Unusually harsh winters, constant fear of Spanish attacks, and politically savvy Native American leaders conspired to deny the English a firm foothold in the Chesapeake Bay. Two other often overlooked factors also got in the way of success: the very nature of colonial designs, and the somewhat marginal place that Virginia occupied in English geopolitical imagination. Karen Ordahl Kupperman deftly sets the history of Roanoke and Jamestown in the larger context of English colonial and trading policies in Africa, the Amazon, the Caribbean, Ireland, and the Ottoman Empire. Her story of the English spills over the Atlantic to incorporate the Middle East. Moreover, Kupperman rejects the historiography that has cast New England and Virginia as two antithetical colonial experiences. By following the career and writings of Captain John Smith, Kupperman persuasively shows that the constant failure to settle Virginia created the conditions for the English to rethink their models of colonization. Jamestown forced the English to give up deeply rooted aspirations to quick wealth by striking

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gold or finding maritime shortcuts to Asia. In light of lessons learned in Ulster, Jamestown slowly ceased being a colony of drifting males led by ruthless aristocrats and became a family-centered, religiously driven, and politically autonomous agricultural community. According to Kupperman, the Pilgrims got their cues from Jamestown, not the other way around. It should be said, however, that Kupperman unwittingly offers evidence that the first successful agricultural English model colony was Bermuda, not Jamestown. Also, Kupperman overlooks the possibility that even more influential than the example of Ireland was the attention shareholders of the Jamestown project paid to the commercial-agricultural success of the Spanish American colonies, which had already abandoned the quick, aristocratic search for gold and shortcuts to Asia.

This is a fascinating, eye-opening book. Kupperman reveals the staggering numbers of cultural go-betweens in the English Atlantic. Over the course of four decades thousands of settlers were incorporated into the Native American polities as they were either abandoned by their backers or willingly given as children for the Amerindians to educate. A lesser number of Amerindians grew up among the settlers or were taken to England; some returned to America to help establish colonies. It turns out that the culturally ambidextrous Pocahontas/Lady Rebecca Rolfe was not unusual but the norm. The heavier traffic one way (of Europeans becoming Amerindians) might explain the greater political sophistication of the natives who for at least four decades kept the English in Virginia off balance.

Roanoke and Jamestown were only two of dozens of other colonial ventures spearheaded by English merchant-adventurers. In fact, merchants had more interest in establishing footholds in (and the literati greater curiosity in understanding) Africa, the East Indies, and the Middle East. Given this context, a key question for Kupperman is why the backers of Jamestown stubbornly kept on sending supplies and settlers even after 1622, when the colony was nearly wiped out by an uprising of Tidewater Indians. Why didn't Jamestown suffer the fate of Sangadhoc on the Kennebeck River in Maine? Originally chartered in 1606 to the Plymouth's Virginia Company, Sagadahoc folded almost immediately after the first ship of colonists was captured by the Spaniards. Kupperman finds in the location and structure of the shareholding company the reason for the longevity of the Jamestown project: the sale of cheap shares (itself a radical innovation) and the resilient depth of the large London market allowed the project to remain commercially viable despite repeated disappointments and poor returns.

Kupperman's short but illuminating analysis of the religious underpinnings of the Jamestown project is also worth mentioning. She not only highlights the sermons that accompanied every new campaign by shareholders to bolster the sagging venture; she also demonstrates the deep support the project encountered among networks of pious Anglican and Puritan shareholders who were intent on converting the Indians. This evidence further undermines the historiography that insists in drawing distinctions between Virginia and New England.

Despite all these contributions, the book reads more like a collection of essays than a sustained argument. In fact, the first half is an exploration of multiple themes in the Elizabethan and Stuart engagement with the larger non-European world. It includes a wide-ranging discussion of English encounters with Africans, Ottomans, and Native Americans and with the landscape, fauna, flora, and climate of the New World. It is only in the second half that Kupperman tackles the history of the Jamestown project (1606–1622) head on. Be that as it may, Kupperman has offered a set of provocative arguments that put the history of early Virginia squarely in the larger context of English Atlantic and global concerns.

JORGE CAÑIZARES-ESGUERRA
University of Texas, Austin

S. MAX EDELSON. *Plantation Enterprise in Colonial South Carolina*. Cambridge, Mass.: Harvard University Press. 2006. Pp. xii, 383. $45.00.

In this book, S. Max Edelson offers a nuanced and beautifully written account of the evolution of plantation society on the margins of the English colonial system. Beginning with the story of the English settlement of South Carolina in the late seventeenth century and ending with the planters' efforts to reconstitute their society following the American Revolution, Edelson traces the environmental, economic, and cultural forces that provided the context for elite colonists' efforts to craft their own identities as landholders and leaders of a society at once immensely profitable and seemingly vulnerable to catastrophic threats. In the best possible sense, Edelson's argument derives methods and insights from the rich secondary literature dealing with colonial slaveholding society. Jack Greene's model of core and periphery is perhaps the most obvious influence on Edelson's efforts to decipher the dynamic by which colonial society operated. Historians will also note the impression made on Edelson's scholarship by authors such as Peter Coclanis, Joyce Chaplin, Mart Stewart, Philip Morgan, and Michal Rozbicki. And yet, for all of Edelson's admirable attention to the historiography informing his topic, his scholarship is, at heart, intensely creative—an at-times miraculous synthesis of macro- and micro-economic analysis, environmental history, architectural analysis, and, of course, inquiry into the power dynamics between masters and slaves.

The early chapters of the book emphasize the flexible and adaptive responses that English entrepreneurs offered to the challenges posed by the early colonial landscape and market. Without ignoring evidence of the economic and political vulnerabilities that resulted from the creation of a slave-based plantation system that focused primarily on the production of rice, Edelson persuasively recounts the dynamic nature of this

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unfolding society, and he takes particular pains to characterize the planters as agricultural innovators. In Edelson's narrative, the settlers were able to improvise a plan for their society that drew simultaneously on English agricultural tradition, the knowledge (and, obviously, the labor power) of African slaves, and the particular opportunities afforded by the Carolina landscape. Edelson never ducks away from the fact that South Carolina slaveholders built a decidedly un-egalitarian social system that increasingly concentrated power into the hands of an entrenched elite. According to Edelson, however, their world view was less reactionary and suspiciously inflexible than it was synthetic and supple. The planters and merchants were actively integrating agricultural knowledge to fit the demands and opportunities offered by the local environment so as to successfully pursue profit and cultural standing in a colonial, transatlantic framework for exchange. Explored from this vantage point, the story of the "discovery" of rice as a commodity receives its very best telling—one that will profit even experts in this field of study.

Data sets painstakingly amassed from sources such as land advertisements in the *South-Carolina Gazette* and the account books of the merchant-planter Henry Laurens enable Edelson to discern the shifting relationship between, on one hand, individual entrepreneurs who sought opportunity for cheap land at the edge of colonial settlement, and, on the other hand, the broader society in which these entrepreneurs hoped to acquire standing. Because South Carolina planters made their fortunes on the margins of the coalescing British Empire and then eagerly attempted to parlay that wealth into cultural and political standing in the metropolitan center of English society, they could never escape the tensions swirling from this duality. Edelson presents the core-periphery relationship as an ever-evolving dynamic informing not just England's relationship with the colony or the coastal zone's relationship with the backcountry but also Charlestown's relationship with subsequently settled zones along the Carolina coast and, for that matter, even the relationship between plantations owned by an individual entrepreneur such as Laurens. Although the findings offered by Edelson's detective work into the primary sources introduce new degrees of complexity into the model of colonial plantation growth, his account is elegantly written and presented with admirable clarity. And for all of his insight into the existing scholarship—insight that makes this book the starting point for future research into colonial plantation slavery in America—Edelson also succeeds on perhaps the most critical level of writing history: he manages to convey the contingencies and drama that informed the choices made by the participants, willing and unwilling, in the process of building a slaveholding society in the Lower South.

In a larger sense, Edelson's argument is a persuasive rejection of the notion that isolation and the rejection of Western modernity informed southern identity from its inception. The twenty-first-century historiography on plantation society has clearly left behind either-or formulations of slaveholder identity that once required scholars to choose between rival theories of planters-as-capitalists or planters-as-paternalistic-opponents-of-individualistic-capitalist-accumulation. Edelson's analysis of the ideological and economic choices made by Laurens reveals how colonial slaveholders consistently juggled the demands of a global market economy with metropolitan cultural cues that raised troubling questions about the planters' ruthless pursuit of profit. There can be no doubt that Edelson's approach to this subject matter—which itself can perhaps be viewed as the crest of the wave of scholarship advancing a transatlantic analytical framework—represents a historiographical advance that has deepened our understanding of the power dynamics informing the system of plantation slavery.

JEFFREY ROBERT YOUNG
Georgia State University

PETER N. MOORE. *World of Toil and Strife: Community Transformation in Backcountry South Carolina, 1750–1805*. Columbia: University of South Carolina Press. 2007. Pp. xii, 175. $34.95.

This is a tidy book. In a little more than one hundred, close-packed pages, Peter N. Moore takes the backcountry South Carolina community of the Waxhaws from its settlement in the 1750s along the Catawba River to the end of the eighteenth century when its farming families had finally "arrived" (p. 77). Arrival came in the form of regional integration with lowcountry Carolina, in commercial agriculture, and in slaveowning. Although short, the story is large, and in Moore's skillful telling, not overstuffed. As a result the Waxhaws joins the growing list of communities in the early American backcountry coming under the scrutiny of varied historiographical approaches that have been "new" at different times during the past forty years. Social history, ethnohistory, environmental history, imperial history, microhistory, Atlantic history, and to a lesser extent cultural history all inform Moore's book. It is, therefore, important.

Immigrant families from the north of Ireland moving south from backcountry Pennsylvania and Virginia were the first to take up land in the Waxhaws (named for a nearby creek and a once-resident Indian people). From the outset their community developed in the dynamic tension of land, kinship, and security. The desire for fertile, well-watered farms dictated dispersion checked primarily by a frontier exchange economy in which kinship governed centripetal patterns of collective self-sufficiency. The threat of Indian attacks escalating in the 1760s also encouraged settlers to keep each other close by. Nearly everyone in the charter generation was Scots-Irish and Presbyterian, although for Moore kinship and economy trumped ethnicity and religion in creating community coherence.

Transformation thereafter brought to bear all the major forces shaping American and Atlantic history for the next half century. Commercial grain markets

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emerging with the expansion of the North American provisions trade in the Caribbean and southern Europe eroded kinship ties as households produced more for profit than for each other. At the same time, population growth led to outmigration, new, more distant sources of credit allowed for capital accumulation or reinvestment, farm households achieved ever higher levels of economic competence, and slaves increasingly replaced family or indentured labor in mixed farming. By 1800, therefore, what once was a southern extension of mid-Atlantic yeoman farm communities now "more closely resembled the slaveholding, staple-producing world of the South" (p. 3).

In multiple ways Moore's book represents the maturation of backcountry studies—hence its added significance. His focus on community coherence and change comes from a mold cast in the forge of the New Social History as the sources of its interest shifted from New England to the Chesapeake and eventually to the colonial backcountry in the 1980s. All the major themes of scholarship on the southern backcountry materialize in the flow of his story. New settlements constituted a vector of disparate forces, one emanating from settler ambitions for self-betterment and the other from imperial interests in securing North American frontiers. Kinship, grain-livestock agriculture, ethnic diversity, religious pluralism, and a society with but few slaves then distinguished the backcountry from the lowcountry. In the case of South Carolina these differences help explain the tensions of the Regulator Movement of the late 1760s and separate backcountry/lowcountry experiences during the American Revolution. Economic growth after the revolution and especially in the 1790s as American commerce expanded throughout the Atlantic world created new economic opportunities in North America, and nowhere more so than in the now older backcountry interior. Capital, credit, and a consumer revolution combined with population growth and outmigration to erode community coherence and allow for new forms of regional integration. In the case of the Waxhaws conflicts arose between new and old settlement communities as social articulation intensified with the adoption of slaveholding among middling farm families. These are stories told in fragments elsewhere in the backcountry but combined with exceptional sophistication and clarity in Moore's account.

This is not to say that little new surfaces in this book. By holding his focus on the Waxhaws, Moore makes more sense out of the chaotic factionalism and partisan fighting during the revolution than broader studies of backcountry political alignments permit. And the concluding gambit on religious controversy between the new, more radical, Covenanting Presbyterians arriving after the revolution and the old established churches that revolved around revivalism, evangelism, and hymn versus psalm singing offers deep insights into a Scots-Irish identity that was more varied, more reflexive of changing times, and more attuned to national and transnational cultural currents than would be allowed by an essentialist view of the Scots-Irish of the sort found in James Webb's recent book, *Born Fighting: How the Scots-Irish Shaped America* (2005). Connecting the dots between Moore's period and the present on the question of Scots-Irish identity will keep historians of the backcountry jumping for some time to come.

WARREN R. HOFSTRA
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W. J. MEGGINSON. *African American Life in South Carolina's Upper Piedmont, 1780–1900.* Columbia: University of South Carolina Press. 2006. Pp. xvi, 546. $59.95.

W. J. Megginson has written a long social, political, and economic history of an ignored subregion of the south, the Upper Piedmont. The far western corner of South Carolina was marked by small farms and a seventy percent white majority in contrast to much of the rest of the state. In this study, Megginson is "intending to encompass rather than to exclude" (p. 9). As a result, readers hoping for a focused thesis will be frustrated. Instead, this sprawling collection of anecdotes and statistics addresses one topic after another, many of which are interesting in their own right. Accordingly, the strength of this study is its coverage. In what must have consumed many years of diligent research, Megginson tape-recorded interviews with fifty-nine subjects, tabulated quantitative sources, and combed over fifty archives, court houses, and museums for data. Many scholars will want to consult the index for their areas of interest.

Echoing a central argument of Orville Vernon Burton's *In My Father's House Are Many Mansions: Family and Community in Edgefield, South Carolina* (1985), Megginson's most frequently repeated theme refers to the dichotomy of the economic and interpersonal aspects of slavery and sharecropping. Whites unabashedly spoke of their slaves as investments. They bought, sold, and mortgaged them and anticipated the appreciation of their value over years. Yet, as Megginson relates, the white people who employed the phrase "my family white and black," although "myopic, paternalistic, and sentimental . . . clearly understood that all peoples on a single estate were linked together in many ways. Blacks and whites jointly provided medical care, food, clothing, and shelter for one another, acting from dramatically unequal positions of power and resources. They shared not only material things but emotions also" (p. 123). For example, about a third of African Americans in the Upper Piedmont joined white churches in the antebellum period, sometimes choosing churches other than the ones their masters attended. Most had ceased to be members by 1870, although some remained. Conversely, in the early years after emancipation, a few whites joined black churches. Megginson offers evidence of a presegregationist culture in housing patterns, recreation, religion, and health care but provides no explanation for its existence or why it evolved into an increasingly segregated one. Was the answer a premodern culture, the demands of neighborliness, a culture of personalism, or shared religiosity? The reader speculates unaided.

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Some of Megginson's stories are startling. He found slaves who kept a tavern in 1846, and a free person of color, Washington Graham, who taught a school attended by some white children in the 1850s. He located Alfred, an escaped Georgia slave, who was lynched in 1862 for preaching that Yankee troops would bring in the millennium and free the slaves. Some of Megginson's observations engage ongoing scholarly debates. He found that living in a district with small slaveholdings stressed black families as many couples lived on separate plantations. But, like Burton, he also found that by 1870, over seventy-five percent of African Americans lived in nuclear families or ones that included extended family.

Megginson's description of the rise and fall of Reconstruction may be the most important section of the book. In it, he adds to recent literature that has emphasized the early success of Reconstruction. Federal troops and the idea of Emancipation arrived late in western South Carolina. One church kicked out a black member for "absconding from his master" after the war (p. 202). As late as 1866, one white woman listed her ex-slaves as assets in a will. But when the Union troops finally arrived in July 1865, most whites accepted the new status quo. For the first three years of Reconstruction, local newspapers encouraged cooperation with federal forces and condemned violence against blacks and soldiers. Local white Democrats seemed willing to work with local black and white Republicans in building a new order. Megginson even found praise of individual black politicians in Democratic papers. According to Megginson, such sentiments indicated only the potential of white cooperation under the best circumstances. In reality, violence against politically and economically assertive African Americans waxed and waned with the presence of Union troops stationed in the area. When they were absent, violence reached frightful levels as Klansmen terrorized black voters, frequently by assaulting women and children. African Americans met violence with violence and played an active role in politics until 1868, when voter fraud, economic coercion, and a massive wave of white violence shrank (but failed to eliminate) black political activity.

Megginson has opened our access to the Upper Piedmont South through this exhaustively researched, encyclopedic study of African American life. While he did not design his book for a broad audience, it will be useful especially to historians of Reconstruction South Carolina, and the Piedmont. In particular, it is a treasure trove of leads for graduate students in search of thesis topics.

MARK SCHULTZ
Lewis University

AMY C. SCHUTT. *Peoples of the River Valleys: The Odyssey of the Delaware Indians*. (Early American Studies.) Philadelphia: University of Pennsylvania Press. 2007. Pp. 250. $45.00.

Among Native peoples of the eastern woodlands, perhaps only the Iroquois have generated more scholarly literature than the Delaware (Lenni-Lenape). Nearly ubiquitous in many histories of the colonial northeast, the Delaware people played a key role in early European and native contacts, interacting with Dutch, Swedish, English, and French colonists and traders, as well as other native peoples. Works such as James H. Merrell's *Into the American Woods: Negotiations on the Pennsylvania Frontier* (1999) emphasized the Delawares' efforts as negotiators and their attempts to create an accommodation between themselves and Europeans, while Gregory Evans Dowd's *A Spirited Resistance: The North American Indian Struggle for Unity, 1745–1815* (1992) stressed their role in secular and spiritual resistance movements in the Ohio Country and the Great Lakes region.

Most scholars of Native North America regard Delaware history as largely a story of repeated dislocations in the wake of European contact, often caused by warfare, disease, and land swindles. These disruptions created another distinctive feature of Lenni-Lenape history by splitting the tribe into eastern and western bands. Few scholars, however, have examined the remarkable ability of the Delaware to repeatedly reconstitute their disrupted communities. Amy C. Schutt examines this aspect of Lenni-Lenape history, tracing their frequent removals and reorganizing of their communities from the early seventeenth to the late eighteenth centuries, as they responded to successive European invasions of the eastern woodlands.

Schutt argues that the ability of the Delaware continually to reestablish their communities depended on the traditional kinship ties that maintained cohesion between Algonquin communities in the Northeast on the one hand, while on the other hand they found themselves creating new alliances with other Native peoples, such as the Susquehannocks. Additionally, European settlement, and the dependency of Native people on European trade goods, dictated that the Delawares forge alliances with different colonial powers.

In the early seventeenth century, Schutt notes, the Delawares formed wary commercial relationships with Swedish and Dutch traders. These rather loose ties were replaced by a relatively strong alliance with William Penn in the later part of the century. Penn often treated the Delawares fairly when he purchased land from them, but his need to establish clear title to lands within his colony is what drove him to extinguish Indian land claims. Future Delaware diplomats would repeatedly invoke their people's mostly positive memories of their dealings with Penn. But if Penn sought to treat Native people fairly, his heirs, as Schutt points out, made no such effort.

The Walking Purchase and its aftermath strained the ties between the Pennsylvania colony and the Delawares, and it exacerbated the division of the Lenni-Lenape into eastern and western bands. Most of the eastern bands remained in the vicinity of Philadelphia

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and New Jersey, while some western bands established themselves in western Pennsylvania, and others went as far west as the Ohio country.

Both the eastern and western Delawares adapted to their new realities. The eastern band pursued several strategies in order to maintain a place for its members in eastern Pennsylvania, establishing for example, an alliance with the Moravian sect in Bethlehem. Although the Moravians hardly thought of their missionary activities among the Lenni-Lenape as an alliance, their assistance proved invaluable when the Natives dealt with colonial authorities. More than once, Schutt points out, the eastern Delawares tried to establish a "reserved space" for themselves among Europeans in Pennsylvania. Schutt also notes that the Delaware leader Teedyuscung took this a step further, seeking to create reserved space between Indians and colonists that would in effect, be a buffer zone.

Some western Delawares chose to ally themselves with the French at the onset of the French and Indian War, but other bands remained neutral, and sought a role as mediators between the English and Indians. Delaware mediators drew on their forefather's memories of their dealings with Penn, and they also invoked their status as "grandfathers" to other Algonquian peoples, such as the Shawnees. By serving the English in this way, the Delaware hoped to be able to recover some of their eastern lands, a vain hope they later transferred to the nascent United States. Schutt ends her book with an epilogue that traces the continuing Delaware diaspora through the nineteenth century, as different bands moved to Wisconsin, Oklahoma, Texas, and Ontario.

Overall, much of the material in Schutt's book is not new to historians of Native America, but it is a useful addition to the growing literature of Delaware history. She delves into great detail regarding Lenni-Lenape migrations, and seeks to explain their ability to reform their communities in the wake of these disruptions.

Roger M. Carpenter
University of Louisiana, Monroe

Sara S. Gronim. *Everyday Nature: Knowledge of the Natural World in Colonial New York*. New Brunswick, N.J.: Rutgers University Press. 2007. Pp. x, 261. $44.95.

One salutary effect of the global-warming crisis is its timely reminder that humans inescapably dwell in a natural world they cannot fully control. Early modern Europeans needed no such lesson, for their daily activities brought them into constant contact with a seemingly capricious nature. Out of that contact came certain assumptions about how the natural world worked: what forces governed its operation, how it shaped the land and people, and so on. But what happened when people with this empirical understanding encountered new ways of thinking about the world? This question lies at the heart of Sara S. Gronim's book. Using colonial New York as a case study, Gronim asks how ordinary colonists adapted to intellectual advances associated with the Scientific Revolution.

The first of the book's two sections, covering the period 1650–1720, discusses the intellectual baggage colonists brought with them, and how it shaped their approach to New World nature. For the most part, their assumptions remained unchallenged. Colonists displayed little curiosity about the qualities of the local environment, presuming that they could easily employ familiar surveying and agricultural technology to reshape it into a reasonable facsimile of the rural Dutch and English landscapes they had left behind. Notions about how illness affected bodies and what remedies should be applied remained intact, as did belief in astrology and Ptolemaic cosmology. Nature in the New World, as in the Old, could be influenced by spiritual forces, ranging from divine providences such as epidemics and comets to witches' maleficence. These notions about the workings of the natural world were widely shared by average folk, and could be confirmed, they believed, by personal experience.

But what happened when experts came along to suggest that new scientific discoveries offered a truer understanding of how the natural world worked? This is the subject of the second part of the book, covering the half-century before the American Revolution. On the whole, ordinary colonists resisted changing their views. They showed little interest in agricultural innovation, mechanistic ideas of the body that undermined humoral theory, or Copernican astronomy. They regarded traveling shows demonstrating electrical phenomena as entertainments rather than educational opportunities. What could not be confirmed by personal, sensory experience remained suspect. So too did the purveyors of the new ideas. Most New Yorkers hesitated to grant automatic authority to self-proclaimed experts, especially in a diverse and factious colony where hopes for economic gain or political advantage—not disinterested knowledge—seemed to inform most peoples' actions.

Resistance to scientific advances was not universal. Even ordinary colonists could embrace smallpox inoculation when experience proved its efficacy, and when preparation for treatment could be reconciled with familiar ideas about balancing bodily humors. For the most part, however, new ideas appealed principally to a small elite more fully integrated than most colonists into transatlantic cultural discourse. More receptive to appeals to reason and a rising faith in progress, such individuals expressed their refined sensibilities by collecting and categorizing plants, reading Isaac Newton, and forging social and intellectual connections through correspondence with like-minded counterparts in Britain.

A final chapter reinforces the theme of colonial ambivalence by examining ideas about maps and racial categories. Although strategic demands of imperial warfare made the production of accurate maps more important than ever, colonists remained too skeptical

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of mapmakers' political motivations to accept their products as objective representations of the physical world. Yet New Yorkers willingly absorbed new ideas about fixed racial categories. They did so, Gronim argues, because recent experiences with slave rebellion in 1741 and brutal Indian conflict during the Seven Years' War encouraged them to regard Africans and Indians as inherently savage.

The uneasy pairing of disparate subjects makes the final chapter the least satisfactory, and also highlights some overall weaknesses. Here, as throughout, Gronim explores ideas held by a rather amorphous group of "ordinary colonists"—a particularly problematic generalization in a religiously, ethnically, and linguistically diverse colony that only grew more diverse between 1650 and 1775. Experience may well have determined the extent of colonists' receptivity to new ideas, but did they all share similar experiences from which to draw similar conclusions? Tracing change over time also presents problems, exemplified by the rather tepid conclusion that New Yorkers' understanding of the natural world was "somewhat different" in 1775 than 1650 (p. 198). If continuity is in fact the main thrust of the narrative, the author should explain why 1720 serves as a meaningful chronological divide.

Although these concerns diminish the book's overall impact, scholars of early American history and the Atlantic world will nevertheless find it useful. The emphasis not only on new forms of knowledge but on the social context within which transmission of knowledge takes place offers an important reminder of the many ways the early modern world differed from our own.

Virginia DeJohn Anderson
University of Colorado, Boulder

Thomas A. Foster. *Sex and the Eighteenth-Century Man: Massachusetts and the History of Sexuality in America*. Boston: Beacon. 2006. Pp. xx, 223. $28.95.

In his study of eighteenth-century masculinity, Thomas A. Foster begins with a familiar premise: residents of Massachusetts did not observe the line between public and private that Americans generally would in following centuries. While this insight can be traced at least as far back as John Demos's *A Little Commonwealth: Family Life in Plymouth Colony* (1970), historians still seem to be finding unnoticed contours of the past revealed in the light of this observation. Foster effectively demonstrates how men had to answer for their sexual conduct within a community of men. Divorces and adultery could embarrass a man and perhaps even call into question his trustworthiness as a business partner. Foster also effectively reveals how eighteenth-century Massachusetts residents did have very clear expectations for sexual performance in men. After reading this book, few historians would dare say any longer that heterosexuality was "invented" in the late nineteenth century.

Foster keeps his focus keenly trained on the sexual aspects of eighteenth-century manhood. This puts his work in contrast to some other recent studies of colonial New England masculinity, such as those by Lisa Wilson and Anne S. Lombard, which emphasize the wider social demands of being a spouse, master, and father. The intensity of focus has real payoffs, for this book gives much-deserved attention to some understudied eighteenth-century male sexual types such as fops, cuckolds, and sodomites. But at times the focus becomes myopic, for it is hard to see why some of the changes Foster describes occur. A fuller engagement with changes in wider eighteenth-century society might have helped Foster sort out the patterns he perceives.

Basing his book upon court records and print sources, especially newspapers, Foster clearly reveals that Massachusetts residents were not oblivious to sexual expressions in wider British society. Notices of raids on molly houses (gathering places in London where men cross-dressed and shared same-sex intimacy) surely attuned waning Puritans to sexual mores at distinct odds with their own. Foster readily concedes that a molly culture did not spring up on American soil, but he nonetheless persuades when insisting that Americans were developing a sense of sexual identities long before the medical discourse of late-nineteenth century sexologists and psychiatrists. Like Richard Godbeer, Foster shows that colonial New England residents were forced to come to terms with men who had lifelong predispositions for sex with other men. Foster insightfully describes how past and present societies alike have competing languages, some secular, some sacred in origin, which prove more or less open to accommodating notions of sexual identity. The sodomite would have been more readily perceived because he stood in marked contrast to heterosexual men who were encouraged to marry and multiply. Massachusetts men took pride in siring children with their wives; men who lingered too long as bachelors became suspect. Rather than being perceived as virile swingers, bachelors were perceived as effeminate "half-men" (p. 101) who lacked sufficient heterosexual desire. Single men could also be feared as sexual predators, rapists, and seducers who might target vulnerable women. The male ideal, then, was the married, self-controlled man who knew how to channel his sexual urges into a growing family.

While Foster does an impressive job mapping the sexual landscape of eighteenth-century Massachusetts, he struggles more when describing patterns of change over time. For example, his book awkwardly straddles the American Revolution, in places assigning it great interpretive significance, in others giving it responsibility for epiphenomena associated merely with the war, and sometimes ignoring it entirely. It may have been the case that the revolution had an uneven impact, but the book needs a more clear and sustained assessment of whether and how the revolution mattered. Certain social changes long associated with the American Revolution, such as the declining power of patriarchs over youth and servants, the growing privacy of the nuclear

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family, and the greater availability of divorce, while acknowledged, deserve fuller interpretive play in this book. For example, when Foster looks simultaneously at charges of rape and seduction in chapter three, he misses an important opportunity to analyze the way the rhetoric of seduction began to squeeze out rape in popular culture in the late eighteenth century. Did this change correlate with growing prerogatives for youth in courtship? Foster also calls our attention to some rather intriguing changes for which he proffers no explanations. For example, he shows that fears of sodomites and black rapists mysteriously disappeared in the mid-eighteenth century. While the evidentiary record might not yield conclusive answers, Foster has studied his topic more deeply than anyone else, so the reader is left yearning for analysis, even if only speculative in nature. Despite such reservations, however, Foster's map of eighteenth-century manhood should become standard reading for scholars interested in sexual identity in early America.

RODNEY HESSINGER
Hiram College

JENNIFER J. BAKER. *Securing the Commonwealth: Debt, Speculation, and Writing in the Making of Early America*. Baltimore: Johns Hopkins University Press. 2005. Pp. 218. $50.00.

This book by Jennifer J. Baker convincingly argues that early American literary culture from the 1690s to the 1790s was heavily preoccupied with questions of finance, from the campaign to establish confidence in government-issued paper money at the end of the seventeenth century to the debates surrounding national debt in the aftermath of the Revolutionary War. Indeed, according to Baker, early American print culture from Cotton Mather to Washington Irving was engaged in a steady campaign of public advocacy devoted to accustoming the fledgling nation to the notion that debt, both public and private, was not only beneficial but also necessary to the project of building a commonwealth. In this era of transition in Europe and its colonies from a republican ideology that held property ownership to be the cornerstone of full citizenship and masculine virtue to a modern economy made up of near universal participation in a range of markets, financial institutions, and credit schemes, it was necessary to convince readers that dependence on creditors and immersion in market relations, rather than disinterested independence from the mercantile domain, were signs of both virtue and patriotism, as well as means of bringing the nation together. The purpose of the literature of this period, she contends, was at least in part to argue for "the value of a good credit rating" (p. 71).

Baker explicitly positions her work against a Marxist tradition that sees the emphasis on the market's capacity to fashion collectivity as one of capitalism's greatest ruses. That this ruse still carries considerable power can be seen in recent coinages such as "the financial community" or "the community of trading nations," although skepticism about capitalism's contribution to social cohesion is already widely evident in the period covered by Baker's book. Herman Melville was only one among a long line of commentators who found wildly implausible the notion that mutual financial obligation could ever function to create collectivity, given the level of self-interest at its heart. Rather than addressing the question of whether capitalism can in fact foster community head on, Baker deftly sidesteps the question by insisting that she wants to make a historical rather than a political-economic point. She wants, she says, to read early American writers "on their own terms, taking seriously their attempts to see a social dimension in an increasingly modern economy" (p. 3). This project of forging community from the mess of market relations, according to Baker, was taken up in works from periodical literature to early novels, from broadside pamphlets to plays and sermons, by writers as diverse as Mather, Benjamin Franklin, Royall Tyler, Charles Brockden Brown, and Judith Sargent Murray, as well as many others. One of the book's greatest strengths is the detailed and beautifully delivered close readings it gives to all of this wide-ranging material, from some of the classics of early American literature to more aesthetically challenged artifacts, including mock-epic screeds on the subject of whether or not paper currency should be backed by metal specie and odes to paper factories and printing presses sporting titles such as "Paper-Mill" and "Typographia." Perhaps because this reviewer is a nonspecialist in the field of American literature, the readings of all of these texts seemed pretty much equally fascinating, and Baker does an admirable job of offering just enough background, both historical and financial, so that anyone with a basic familiarity with eighteenth-century literature and an interest in literary history, the history of print culture, or the relations between culture and the market in this period will find her book an engaging read.

Baker lets key terms such as speculation, imagination, faith, value, and credit drive her analysis, and the recurrence of these terms lends weight to her claim of an ongoing preoccupation with money matters in the literature of the period. She is also interested—in a less fully explored but no less engaging theme—in drawing an analogy between fiction and paper instruments of credit such as banknotes, both of which, she argues, depend on a "deferral of disclosure" (p. 134). One would have liked to hear more about the claim that "financial deferral has a narrative counterpart" (p. 134), and more attention to developments in British and French thinking about debt and speculation in this period would have been welcome, too, as a means of testing the distinctiveness of the American case. Nonetheless, this book's virtue lies in its willingness not to belabor a point, as well as in its extremely graceful way of offering correctives to existing readings of well-known works such as Franklin's *Autobiography* without fanfare or

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grandstanding. To finish an academic monograph wanting to read more is surely a good—and rare—thing.

JODY GREENE
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JOSEPH T. GLATTHAAR and JAMES KIRBY MARTIN. *Forgotten Allies: The Oneida Indians and the American Revolution*. New York: Hill and Wang. 2006. Pp. 434. $30.00.

Joseph T. Glatthaar and James Kirby Martin have made a signal contribution to Native American history and the history of the American Revolution in this detailed study of the Oneidas, one of the Iroquois Six Nations. The story of Oneida allegiance to the American cause, how they suffered greatly at the hands of pro-British Mohawks, and the tragic loss of most of their land after the American Revolution is not new. Barbara Graymont's *The Iroquois in the American Revolution* (1972), recent dissertations, and specialized monographs and journal essays have provided the general outline of events, but this book is rich in detail and sweeping in its contextualization of the Oneidas' revolutionary experience.

In weaving the story of the Oneidas' attempt to maintain neutrality while other Iroquois nations were throwing in their lot with the British, the authors rightly cast the sweet-tempered missionary, Samuel Kirkland, in a central role. Then, as the Oneidas were forced into a combatant posture in order to defend their homeland against the incursion of the British and their Six Nation allies, Native leaders such as Johannus Skenandoah and Good Peter, in the retelling by Martin and Glaathaar, step onto the stage. It was in this period, during the second and third years of the war, that several score of Oneida warriors followed American officers to fight at Saratoga and join the winter encampment at Valley Forge in the winter of 1777–1778. Passed down in Oneida oral history, the shedding of blood for the American cause is enshrined on Oneida reservations today. The Oneidas are eager to have other Americans know these stories of their "forgotten allies," as this book is titled. To this end, one group of Oneidas has pledged a generous amount of money to the Valley Forge Museum of the American Revolution (still in the planning stage) so that the Oneida blood sacrifices for the American cause will live on in public memory.

Like the Catawbas and Stockbridge Indians—the former's revolutionary experience was beautifully portrayed two decades ago by James H. Merrell in *The Indians' New World: Catawbas and Their Neighbors from European Contact through the Era of Removal* (1989)—the Oneidas found no gratitude at all from the victorious Americans. At first it seemed otherwise, for in the 1784 Treaty of Stanwix the Continental Congress guaranteed the Oneida their ancient homelands as a reward for fighting on the American side. But with the Congress disbanding in a few years, the state of New York moved in. A few American friends, notably General Peter Schuyler, were no match for the land speculators, with the state standing behind them. Nor was their Presbyterian missionary friend Kirkland of much help in preventing the ejection of the Oneida from their homelands. As for Good Peter, he too could do little to prevent Oneida society from spiraling down to a pitiable existence. Like the Stockbridge Indians, who also fought with the Americans, most of the Oneidas moved west to the Thames River area in Ontario, Canada, and to the Green Bay area of Wisconsin.

While rich in detail on the Oneidas' revolutionary experience, this book is less satisfactory on the postwar debacle, leaving the reader with a hazy sense of how the shameful dispossession of the Oneida homelands might have been averted. It is clear that the state of New York paid the Oneidas $6,500 and an annual annuity of $600 for 5.5 million acres in violation of the Treaty of Stanwix. But readers would benefit from a detailed explanation of how the Oneida leaders acceded to this or what interest groups in New York rammed through this tawdry final solution to the fate of the Americans' Oneida revolutionary compatriots.

GARY B. NASH
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MARKUS HÜNEMÖRDER. *The Society of the Cincinnati: Conspiracy and Distrust in Early America*. (European Studies in American History, number 2.) New York: Berghahn Books. 2006. Pp. vi, 212. $75.00.

The Society of the Cincinnati was—and still is—a veteran's organization of the American Revolution's Continental Army officers and their descendents. Seemingly an innocuous group, it nonetheless excited serious concern during the 1780s. The Cincinnati have inspired plenty of scholarship, most recently by Minor Myers, Jr., who wrote *Liberty without Anarchy: A History of the Society of the Cincinnati* (1983). Myers's book is a basic history of the organization. Markus Hünemörder has concentrated on the various complaints and fears that the Cincinnati generated soon after its creation, making his book a useful addition to the historical literature. He seeks to determine if the critics were serious or just paranoid.

As the American Revolution was finally winding down, the Society of the Cincinnati was born. Specifically, it was formed because the army officers wanted their back pay or some equivalent. The author delves into one of the murkiest events of the revolution, the alleged coup attempt at Newburgh, New York, in 1783. Hünemörder falls into the historical camp that believes no real coup was planned. Yet such an opinion ignores the ambitions of men such as Horatio Gates, and world events since decolonization started extensively in twentieth-century Africa and Asia. Many new, revolutionary regimes—as the United States was in 1783—gained independence and expressed their faith in a republican form of government—as the United States also did. Nonetheless, one new nation after another fell under

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the dictatorial control of military men or, in the case of Ghana, under the dictatorial control of the very man who had led the campaign for independence. Earlier revolutions in France and Latin America suffered the same fate. George Washington is the reason why the United States moved along a different path. Washington, the only American general with the ability to seize power, rejected any kind of rebellion against the American Congress.

Washington and other Americans of the eighteenth century were schooled in classical history; he imitated the behavior of an ancient Roman, Cincinnatus, who returned his power back to the Roman Senate and went back to his farm. Continental Army officers created the Society of the Cincinnati as a fraternal order and as a way to keep together to make sure the penniless Congress would honor its financial obligations to them.

The Cincinnati, however, inspired criticism that, despite their classical name, their real purpose was to subvert the very republic they had helped to gain independence. The Society adopted hereditary succession and created a badge for its members to wear. Heredity suggested a reversion to a detested relic of monarchism, while the Cincinnati's badge seemed reminiscent of various orders of knighthood created by European kings.

Americans of the eighteenth century were quick to see conspiracies everywhere; they had inherited such a suspicious nature from earlier English Whigs and Tories whose writings were still popular in America. And the suggested conspiracies were dark indeed: the Cincinnati wanted to make themselves nobles, and they were even compared to druids. Washington, the Cincinnati's president-general, concerned about his reputation being destroyed, tried to push through reforms. Washington's targets included the provision for hereditary membership, but none of the reforms were accepted by all the various state branches and so they were not implemented.

Hünemörder studied a considerable number of the Cincinnati's critics, including major figures such as John Adams, Benjamin Franklin, and Thomas Jefferson. But the most important critics were lesser-known figures such as Aedanus Burke of South Carolina, whose pamphlet *Considerations on the Society or Order of Cincinnati* (1783) really ignited the controversy. Elbridge Gerry of Massachusetts wrote many letters about the Cincinnati that further stirred opposition. A critic in Connecticut, Benjamin Gale, feared that the Cincinnati doomed America to terror in the Apocalypse soon to come. And the Count of Mirabeau spread anti-Cincinnati fears to Europe when he translated, plagiarized, and added to Burke's pamphlet (the Cincinnati had a French branch, too, which was seen in Europe as a threat to monarchy). Despite such concerns, the author believes the Cincinnati had little influence on the Federal Constitution of 1787.

Referring to the work of Richard Hofstadter, Hünemörder concludes that the Cincinnati's foes were not paranoid. Many readers will disagree with that conclusion. Perhaps the most unsettling point Hünemörder makes is that Jefferson was still complaining about the Cincinnati in 1824.

PHILIP RANLET
Hunter College

RONALD L. HATZENBUEHLER. *"I Tremble For My Country": Thomas Jefferson and the Virginia Gentry*. Foreword by STANLEY HARROLD and RANDALL M. MILLER. (Southern Dissent.) Gainesville: University Press of Florida. 2006. Pp. xii, 206. $55.00.

George Washington, a visionary and a nationalist, hoped that citizens of the young American republic would adopt a truly national perspective and embrace an energetic, dynamic, centralized federal government. The interests of Americans, Washington held, might be different on local matters but should be the same on all the essential concerns of the nation. Sadly he came to see that the political attitudes of many Virginians often reflected the confined, provincial interests of their state. And among those Virginians was Thomas Jefferson.

In his excellent new book, Ronald L. Hatzenbuehler argues that Jefferson "never fully devoted himself to national issues" (p. 6). Indeed, the sophisticated Jefferson, so cosmopolitan in his tastes, ultimately felt most at home at Monticello. "French wine always tasted best at Monticello," Hatzenbuehler writes, "where he diluted it with water fetched by his slaves" (p. 7).

Hatzenbuehler explores Jefferson's relationship with Virginia, the state he usually referred to as his "country." It was a complicated relationship, for Jefferson was critical of Virginia yet also bound to its traditions; he was disappointed with the insularity and self-protectiveness of the members of Virginia's gentry, yet was also influenced by their values.

In richly detailed, well-researched chapters, which are organized chronologically, Hatzenbuehler studies Jefferson's sense of Virginia history, the influence that George Mason had on his *Summary View of the Rights of British America* (1774) and the Declaration of Independence, the structure and meaning of his *Notes on the State of Virginia* (1781), and the impact of his identification with Virginia on his early political missions, his presidency, and finally his retirement years.

In the early stages of his political career, Jefferson was both admiring and critical of his native land. He saw that Virginians were capable of accomplishing great things but also expressed disappointment in their failures to live up to the possibilities for progress and change. He scorned the backward agricultural practices of Virginians, encouraging them to abandon the cultivation of soil-depleting tobacco and instead grow wheat and other grains. He deeply disliked the many antidemocratic features of Virginia's constitution. He lamented the state's support for the Anglican Church, the lack of a system of universal public education, and Virginians' complacency about slavery.

While Jefferson dissented from the planter elite on these and other issues, he also found himself under the

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gentry's sway. Especially after his retirement from the presidency, Jefferson's identification with the Virginia elite grew deeper. He retreated from some of the nationalistic and idealistic positions he had earlier embraced and lost a burning desire to reform Virginia society.

In retirement, as he became caught up in the routine of plantation life, his behavior and many of his ideas conformed to those of other wealthy planters. Whereas the young Jefferson had a plan for universal education in Virginia, toward the end of his life he abandoned that plan and concentrated only on the creation of a new state university, designing its buildings and establishing its curriculum. He also declined to play an active role in the search for solutions to the wrenching problem of slavery. "I willingly acquiesce in the institutions of my country, perfect or imperfect," he wrote in 1824, "and think it is a duty to leave their modifications to those who are to live under them" (p. 134).

Jefferson could have done more to change Virginia society, Hatzenbuehler comments, adding that "the gentry, for their part, could hardly have done less" (p. 6). As Hatzenbuehler demonstrates, wealthy Virginians were primarily concerned with protecting their own privileged position, and Jefferson more and more seemed to identify with their interests. This balanced and subtle study is a welcome addition to Jefferson scholarship.

SUSAN DUNN
Williams College

EVA SHEPPARD WOLF. *Race and Liberty in the New Nation: Emancipation in Virginia from the Revolution to Nat Turner's Rebellion*. Baton Rouge: Louisiana State University Press. 2006. Pp. xxi, 284. $45.00.

Historians of the United States not uncommonly suggest that the revolutionary era was an age of radical possibilities. With talk of liberty in the air, the significant hierarchies that shaped the colonial world came under attack such that, whatever the limitations and retrenchments of the years following independence, the contours of a considerably different America became visible. When it came to slavery, for example, every state down to Pennsylvania set itself on course to eliminate human bondage. The impulse for freedom could be seen even as far south as Virginia, which had more slaves than any state in the nation yet was inspired to pass a liberal manumission law in 1782 that encouraged morally awakened masters to act upon their broad-minded convictions, swelling the free black population of the commonwealth.

It is impossible not to wonder, however, just how much wishful thinking underlies the premise that Virginia stepped anywhere near a potentially radical path in the early republic, given that the Old Dominion still had the largest slave population in the United States at the outbreak of the Civil War. Indeed, Eva Sheppard Wolf suggests that the usual story of Virginia slaveholders moved by the American Revolution to free their slaves overstates and misstates the reality. Tracing the history of race and slavery in Virginia over the course of several generations, Wolf acknowledges that there certainly were white Virginians opposed to slavery in the 1770s and 1780s. She makes the case that Quakers played particularly important roles in pushing the state government to pass the 1782 legislation, and that its passage did indicate the presence of a broader ambivalence about slavery and of a climate receptive to new ideas about the institution. But concomitant with liberalized manumission were other laws clearly affirming and underscoring the dedication of Virginians to slaveholder property rights and to a racialized understanding of citizenship. Moreover, Wolf's examination of manumission deeds and wills reveals that the number of Virginians actually motivated to free their slaves as a matter of religious or egalitarian principle was quite small and decreased substantially after an initial burst in the 1780s. Instead of masters ridding themselves of their enslaved property from a sense of spiritual or ideological obligation, the larger number of slaveholders who freed slaves by the 1790s more commonly used the prospect of manumission as an incentive to get slaves to work harder or gave it after they died as a reward to a favored slave or two. Far from challenging the existence of slavery, freedom granted in exchange for loyalty and service was freedom grounded in the prerogatives of mastery and the discipline of enslavement. If anything, freedom for the select few arguably bolstered the enslavement of hundreds of thousands of others.

The manumission law of 1782 was rolled back in 1806 and replaced with a much harsher code that forced slaves freed after its passage to leave the state, but Wolf concludes that this reactionary landmark had little to do with rising anxiety among white Virginians that antislavery had gone too far. On the contrary, with emancipationist sentiment in the early nineteenth century largely the purview of religious outliers and a small secular elite, Wolf argues that white Virginians were less nervous about slavery than they were about racial order more generally. Particularly for whites living in eastern parts of the state, where the declining tobacco economy produced a white outflow and a corresponding rise in the black proportion of the population, the tumult of the American Revolution and events like the Haitian Revolution and Gabriel's Rebellion yielded a sense that white supremacy itself was becoming unstable and that free blacks in particular were a dangerous source of racial confusion best contained or eliminated. Many thousands of free blacks remained in Virginia, of course, and many even seemed estimable or useful enough that whites in their communities supported their petitions for exemptions to legal removal provisions. But whether they stayed in the state legally or illegally, the tensions presented by white desires to correlate race precisely with freedom made the status of free blacks both anomalous and ambiguous, and their liberty severely constricted.

By the end of the 1820s, the environment for free

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blacks in Virginia had become so hostile that, ironically, it became enfolded into a new form of antislavery, one grounded less in concern for black rights than for white rights. Where most white Virginians had supported slavery in the late eighteenth century in part because it provided a relatively neat racial divide where all whites were free, by the Jacksonian era slavery had come to divide white Virginians as non-slaveholders began to insist more forcefully that white freedom ought also to mean white equality. Especially in newer western parts of Virginia less dependent on plantation slavery, whites chafed against property requirements for voting and the disproportionate representation allotted in the state legislature to eastern counties, both of which owed their existence to the influence of slaveholders who claimed that wealth entitled them to power and privilege beyond their numbers. Westerners' complaints forced a new state constitution on eastern conservatives in 1830 and, in the aftermath of the Southampton Rebellion just over a year later, even secured debate on a plan for gradual emancipation and the ultimate exile of black people from Virginia altogether. Given her argument about the limitations of the revolutionary era, it seems only fitting that Wolf conclude with a demonstration of how constrained the racial imagination of white Virginians remained more than fifty years later. Whether they supported slavery or opposed it, almost none could envision a society where blacks and whites might live together in freedom.

Joshua D. Rothman
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Patrick J. Jung. *The Black Hawk War of 1832.* (Campaigns and Commanders, number 10.) Norman: University of Oklahoma Press. 2007. Pp. xii, 275. $29.95.

Historian Robert A. Ferguson tells us that every culture has five or six stories it repeats over and over again as part of its process of self-definition. That has most certainly been so for Wisconsin and Illinois, where, over the past 175 years, the story of the Black Hawk War has been retold more times than easily counted in plays, novels, epic poems, and biographies as well as historical monographs and scholarly articles. Although an armed conflict of less than four months' duration, in which fewer than one hundred Americans were killed, it has long been the source of rousing blood and thunder tales, but in the hands of good historians it has also revealed a good deal about this county's troubled formative development.

Patrick J. Jung's reiteration of this story is indeed the work of a good historian who has rigorously researched his subject and carefully examined the causes and consequences of the tragic little war that rocked the upper Mississippi River region during the stormy summer of 1832. Even after all this time, Jung has managed to come up with new information and insights, adding detail and depth to what we already knew and enhancing the clarity of what we may have only suspected. His book not only improves our understanding but provides us with a well-written, fast-paced narrative with power to evoke strong feelings of pathos and empathy while judiciously steering clear of moral outrage.

Jung sees Black Hawk as the final inheritor of the pan-tribal Indian resistance movement, driven by an angry, anti-settler ideology, which began at the time of Pontiac and was continued by Tecumseh and the Shawnee Prophet. By 1832, however, when Black Hawk and his Sauk followers took up the cause, the movement had been sapped of much of its strength by proliferating intertribal conflicts and acrimonious divisions within the tribes themselves. When their attempt to defy the U. S. government in northwestern Illinois failed to attract support from other regional tribes or from the British in Canada, their dreams of power were shattered and in the end most of them were hunted down and massacred without mercy by the Illinois militia and the U. S. Army.

This book focuses mostly on military matters and makes some valuable contributions to our understanding of that dimension of the struggle. Nevertheless, some aspects of the study warrant questioning. For example, some criticism of the American commander, General Henry Atkinson, seems inappropriately harsh. Indeed, Atkinson has long been a favorite punching bag for critics eager to find someone to blame for the embarrassing failures of American manhood that frequently occurred during the campaign. Jung characterizes the general as incompetent, indecisive, and irresponsible. In so doing he appears to misunderstand Atkinson's intention of being a peace maker, striving to prevent a war rather than, like Illinois governor John Reynolds, instigating one in order to inflate his own personal reputation and fame. Because he was cautious and restrained Atkinson is here judged to be wanting as a warrior.

By contrast, Black Hawk is let off far too easily for deserting his own people on the eve of their greatest peril. The night before the Battle of Bad Axe, Black Hawk and the Winnebago Prophet left the band to seek safe refuge among the Chippewa to the north. Those remaining behind felt bitterly betrayed and Wee-sheet, a civil chief among them, declared, "Now they have brought us to ruin and lost us our women and children, they have run to save their own lives" (p. 180). Nevertheless, Jung offers only unpersuasive excuses for that self-serving behavior.

In some places claims are inadequately supported by the sources. For example, in describing the brutal nature of the Battle of Bad Axe, Jung asserts that there is information "that strongly suggests that sexual assaults [by American soldiers upon Sauk women] may have occurred" (p. 173). He then cites a passage written by Lieutenant Philip St. George Cooke and states that during the battle Cooke came upon a "naked" Sauk girl who may have been "a victim of sexual assault" (p. 173). But in the original source itself Cooke never described the girl as being "naked," nor did he suggest she may have been molested or raped. There was plenty that day

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that was disgusting and cruel. There is no need to try to make it worse than it actually was.

In spite of such issues of interpretation, this is a fine book sure to deepen our awareness of the violent U. S. past by demonstrating once again how Native people paid the ultimate price so that our way of life might prevail.

KERRY A. TRASK
University of Wisconsin, Manitowoc

BRYAN WATERMAN. *Republic of Intellect: The Friendly Club of New York City and the Making of American Literature*. (New Studies in American Intellectual and Cultural History.) Baltimore: Johns Hopkins University Press. 2007. Pp. xiii, 318. $55.00.

Bryan Waterman's recent book speaks to a number of audiences. To those interested in communication studies, Waterman offers a close analysis of citizens imagining and constructing spaces where public issues could be discussed. To literary enthusiasts, Waterman offers a detailed portrait of Charles Brockden Brown's intellectual cohort, their importance in his development, and their influence on his work. To intellectual historians, Waterman offers a clever explanation of how several developing disciplines (law, medicine, literature, philosophy, natural and political science) came together in and were promoted by a network of early American literati joined in their commitment to secular, friendly conversation. To the devotees of American history and politics, Waterman offers a counternarrative to the common story about the American republican political scene.

Seen through Waterman's eyes, the Friendly Club of New York City looks like a substitute for (perhaps a "counterpublic" to) the noisy republican public sphere comprised of partisans discoursing in the parochial vocabularies of national, state, local, religious, and party allegiance. Instead, we see a group of educated professionals attempting to transcend their provincial investments by shedding religious commitment and by pursuing greater knowledge.

Over the course of Waterman's narrative, the reader is treated to charming and informative vignettes that feature many leading early American intellectuals, such as: Elihu Hubbard Smith, Samuel Latham Mitchell, William Johnson, James Kent, William Dunlap, Theodore Dwight, and, of course, Brockden Brown. We get entertainingly presented and eruditely documented accounts of Hubbard Smith's efforts to document the yellow fever epidemic in New York; Dunlap's efforts to allay hostile response to his treatment of George Washington in the play *Andre*; Kent's attempt to make legal practice both a professional and a literary endeavor; many intellectuals' efforts to integrate the writings of William Godwin and Mary Wollstonecraft into an otherwise "conservative" American federalism; and much more.

Waterman divides his book into two major sections. The first three substantive chapters, grouped with a prelude and labeled "Part One: Associations," discusses the protocols and the philosophical underpinnings of Friendly Club interaction. Waterman surveys materials that expound upon the Friendly Club philosophy. He then holds these ideals up to practice by exploring Friendly Club members' behavior during two controversies: the Bavarian Illuminati scare and the introduction of British Jacobinism to American audiences. Part two of Waterman's study turns to what he calls "industries of knowledge," those official places where Friendly Club members tried to exert *their* public influence through emerging professional outlets. In this section, Waterman offers one of his more provocative arguments. He claims that literary societies, particularly the Friendly Club, were a place for professionals to develop authority based on their superior and presumably disinterested knowledge. Outside the Friendly Club's warm and inviting salon, members continued to claim expertise by contributing to an inchoate "protodisciplinary knowledge culture" (p. 11).

The first part of Waterman's study shows us the intellectualized cosmopolitanism that made possible the Friendly Club's republic of intellect, their belief in themselves as "inheritors of the Enlightenment republic of letters, which had ideally transcended politics and nationalism" (p. 31). The second part of Waterman's study shows how these once transcended souls descended back into the messy foray of local politics and provincial minds, trying to articulate their hard-won knowledge through disciplinary avenues, desperately crying for and sometimes successfully claiming "the authority of philosophers and scientists." In many regards, Waterman tells the story of a brilliant social star illuminating the dark void left by the eighteenth century's dying "elite clergy" but ultimately made dim when a "democratized religious authority" flooded the sky (p. 237). One cannot overlook Waterman's palpable disappointment as he explains that the Friendly Club could not pressure American citizens to transcend their nationalism, their religious commitments, their partisan affiliations. In the book's coda, Waterman's tone becomes positively tragic as he narrates Brockden Brown's decision to abandon the *Monthly Magazine* and the mission of an American republic of intellect. We are left with the sour grapes of Waterman's concluding remark that, while the Friendly Club failed to create a secular space for the disinterested pursuit of knowledge, at least they learned "that print culture and professional reputation were useful allies in their efforts to disseminate knowledge" (p. 239).

His affection for the Friendly Club's endeavor makes Waterman's book charming, vibrant, even persuasive. But this affection does lead Waterman to neglect other stories about the Enlightenment's dark side, narratives that focus on racism masquerading as intellectual superiority; classism and intellectual elitism suppressing or warping the American democratic experiment; imperialism parading in the drab taupe of cosmopolitan transnationalism. I mention this not to point out a flaw

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in Waterman's study, but to deliver an aside in an otherwise positive evaluation: there are other stories to be told about the Friendly Club and about the Enlightenment effort at a republic of intellect.

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CAROLINE WINTERER. *The Mirror of Antiquity: American Women and the Classical Tradition, 1750–1900.* Ithaca, N.Y.: Cornell University Press. 2007. Pp. xii, 242. $35.00.

The seven chapters of Caroline Winterer's book trace the important role of "classicism" in the lived experience and careful self-presentation of American women from the mid-eighteenth century up to 1900, when the cachetof Greece and Rome gave way to science and social science. A mélange of supposed classical ideals with specific application to women, developed in England in the eighteenth century, was imported and altered on this side of the Atlantic to address republican circumstances, and changed again as the American polity became increasingly democratic.

Winterer argues that while classicism always had elite implications, its rich reserves of material allowed for great flexibility within this privileged community. At once ornamental and intellectual, the archive of Greek and Roman literature, history, mythology, and art was diversely pressed into women's service. Without disturbing the male-female hierarchy or upsetting conventional notions of women's place, classical versions of the female helped four generations of American women gain respect and influence in public life. In the revolutionary and postrevolutionary periods, the ideal of the "dignified, stoic, and selfless" Roman matron (something like but not identical to the better-known "republican mother" or "republican wife") gave elite woman "a political voice through close and familiar connections to patriotic leaders" (p. 67). Although restricted to the household, she opened the door to new possibilities and especially "to discussions about women's place in the new republic" (p. 101). Early in the nineteenth century, as political debates swirled around the rising standard of living and the pernicious materiality it might encourage, a new language of Grecian luxury, opposed both to supposed earlier Roman severity and contemporary ostentation, allowed women to present themselves as "mothers and buyers, republicans and consumers, Roman matrons and Grecian goddesses" simultaneously (p. 140). Although the "local, familial impulses that drove a few generations of early American women to identify deeply with Greco-Roman womanhood" (p. 164) vanished in a more public female culture, classicism remained useful. There arose "a radically new sense by a few women of how the women of classical antiquity could be used to reform self and society" (p. 164)—a radically new sense leading directly to female Abolitionism. Then, after the Civil War, and in a far more somber key, women found in figures like Antigone a representation of their own patriotic suffering.

Winterer names many women for whom classicism formed an important repertory, including Abigail Adams, Lydia Maria Child, Margaret Fuller, Sarah Grimké, Frances E. W. Harper, Louisa McCord, Dolley Madison, Judith Sargent Murray, Sarah Pierce, Elisabeth Stuart Phelps, Catharine Maria Sedgwick, Margaret Bayard Smith, Mercy Otis Warren, Emma Willard, and Phillis Wheatley. There is perhaps more name-dropping than depth analysis; Winterer wants to demonstrate the pervasiveness of classical allusions in women's familiar writings and the ease with which women resorted to them. The frequent use of terms like "elite" or "a few women" registers Winterer's understanding that it has become impossible to use the word "woman" without modifiers to account for class, race, and other factors; but the limited availability of the classical repertory does not, she argues, make it less worth studying. Elite women were women (and people) too.

Curiously, some of Winterer's examples contradict her claim. The bondwoman Phillis Wheatley was not elite; the abolitionist Lydia Maria Child—daughter of a baker and mainly self-educated—was at best middle class. I find the book strongest on the immediate postrevolutionary era, where women exchanged recondite allusions with classically educated men, and signed their letters and published essays with Roman pen names: Portia, Constantia, and so on. On the one hand, there was talk about matronly virtue; on the other, there were revealing Grecian gowns and luxurious home furnishings. These effectively synthesized interconnections are demonstrated not only by Winterer's graceful prose but also by the book's excellent illustrations.

Winterer is also fine in her coverage of the many ways through which women might have become acquainted with Roman and Greek history and artifacts—mostly by reading men's books. Her study serves therefore as a general history of classicism in the early United States as much as of a female version of classicism. The women's appropriation of a supposedly male-centered practice involved modifying and giving it nuance rather than constructing something wholly new.

The claims for centrality of classicism in antislavery may be overstated. Ideals of Enlightenment rationality (also key to the concept of the "republican mother," the spread of female education, and other trends that Winterer attributes solely to classicism), Romantic individualism, and a Christian moralism initiated by Philadelphia Quakers before New England Unitarians took the Abolitionist lead would seem to have been equally or more important in that movement. Still, possible overstress of the chosen variable is more than balanced by the powerful presentation of classicism in early American women's world view.

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University of Illinois

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CHARLES CAPPER. *Margaret Fuller: An American Romantic Life*. Volume 2, *The Public Years*. New York: Oxford University Press. 2007. Pp. xxi, 649. $40.00.

In her essay on critics in the first *Dial* issue, Margaret Fuller reserves the highest praise for those individuals who embody both apprehensiveness and comprehensiveness. In addition to their ability to "enter into the nature of another being and judge his work by its own law," she asserts that, "having done so, having ascertained his design and the degree of his success in fulfilling it, thus measuring his judgment, his energy, and skill, they do also know how to put that aim in its place, and how to estimate its relations." If we approach Fuller's criterion with the "ironic empathy" that Charles Capper employs when considering her "Romantic language and transcendent spiritual hopes" (p. ix), we might apply these critical qualities to the biographer himself. In the long-awaited second volume of his biography of Fuller, Capper reveals both an apprehensiveness and comprehensiveness worthy of her expansive life.

Anything short of this breadth and depth would have produced a fragmented biography, one that risked breaking apart Fuller's last decade into distinct "eras" without capturing the cosmopolitan striving that informs the "law" of her evolving personal and professional commitments. As Capper suggests in the preface, the major periods from 1840 to 1850—Fuller's *Dial* editorship and "Conversations," her western tour and influential role as the *New-York Tribune* literary editor, and, finally, her European travels and foreign correspondence—reveal a core cosmopolitanism. (That these particular "eras" warrant distinct volumes in and of themselves speaks to the magnitude of Capper's biographical task.) To borrow from Romantic vocabulary in Fuller's *Woman in the Nineteenth Century* (1845), the unfolding of her causal soul speaks to an expansiveness that contains but is not contained by Boston, New York, or even the revolutionary spirit of Europe's intellectual and political landscape. Arguing that past biographers portray Fuller as arriving in Europe "just in the nick of time, allowing her to find the wider experiences she had tragically missed in the United States," Capper instead asserts that she came "well prepared to be provoked by and exploit what she encountered" and that she was welcomed by major literary and political figures of Europe because she "appeared to be neither a Transcendentalist nor even much of an American, as they imagined those tribes to be" (p. xvi). Perceiving Fuller through European eyes is a healthy perspective shift, especially when readers, no matter the scholarship of the past two decades, might still frame her importance within the legacy of New England Transcendentalism.

Moreover, at its best, this volume does not overlook how Fuller's frailties inflect and often enrich how we understand her contributions as a writer and critic. Capper's exhaustive journey through her private and public writings captures the despair, self-doubt, and self-pity that marked her passionate and increasingly political engagement in the world. In the end, how Fuller publicly delineated the implications of gender and notions of the feminine emerges from an accurate rendering of how, in fact, she lived a life of ideas amid private discourse with intimate friends. If the careful enactment of the interaction between Fuller and Ralph Waldo Emerson, for instance, occasionally slows narrative momentum, it also points to the book's strength. Without the painstaking scholarship, we would miss the depth of one of the richest intellectually reciprocal relationships in American literary history, an association too often simplified in Emerson's or Fuller's favor. And, more broadly, we would not see how Fuller's later romantic relationships as well as her influential assertions concerning women's social position and gender androgyny were rooted in a clarifying estrangement and reconciliation with her friend.

One of the most compelling aspects of the biography, however, arises in the chapters tracing Fuller's European tour and especially her time in Italy. In this latter part of the book, Capper more than narrates the many connections with literary and political figures; he draws on extensive research to provide an authoritative context for Fuller's central role in the Italian Revolution. Moreover, with his discovery of documents concerning when Fuller gave birth to her son and when she married Giovanni Angelo Ossoli, Capper finally confirms that she did indeed have a child out of wedlock and then marry, thus correcting past biographical inaccuracies. The account of how Fuller negotiated the striking difficulties surrounding this personal crisis, including temporary separation from her child and her partner while taking official charge of nursing wounded soldiers, truly does live up to the book's title. Yet Capper does not succumb to melodramatic storylines because he apprehends his subject, a woman whose actions grew from deep and well-considered convictions.

Although further illuminating her contributions to Transcendentalist thought and the New York literary and social scene, this biography is especially significant in establishing Fuller's position as one of the nation's foremost transnational intellectuals. Capper portrays dimensions of Fuller often overlooked and, in doing so, offers a remarkable rendering of an apprehensive and comprehensive life.

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LEN GOUGEON. *Emerson and Eros*. Albany: State University of New York Press. 2007. Pp. x, 268. $35.00.

Champion of originality and self-reliance, enemy of imitation and dependency, Ralph Waldo Emerson would be horror-struck by the weight of hagiography devoted to him and the extraordinary influence he is said to have had on the nature and function of American life, letters, and politics. The Sage of Concord, despite much work done to reclaim him as a specific intellectual working in a specific era and under specific conditions, remains unbounded by mere space and time, invoked as *Grundlage*

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of any number of decisively different American objects and pursuits, whether philosophical, social, political, or poetic. Indeed, so generative has been Emerson that he cannot be understood as mere storm-force intellect but only as a whole mental climate. It is this very "everywhere-by-being-nowhere" quality of Emerson that allows him to be proposed as the progenitor of any and all things: liberalism, conservatism, environmentalism, self-helpism.

Len Gougeon's work on Emerson, by contrast, has been pegged to the historical record in a way much other work in the Emerson industry is not. Many readers will be surprised to learn that, far from a remote intellect on the periphery of a turbulent nation, Emerson was an active and courageous lecturing provocateur, a man whose unwavering commitment to the progressive potentiality of the Declaration of Independence (with its proclamation of self-evident equality of all men) put him at constant risk of social opprobrium and, occasionally, physical violence during the quarter century leading up to the Civil War. In a previous book, *Virtue's Hero: Emerson, Antislavery, and Reform* (1990), Gougeon used extensive documentary evidence to argue that the reason Emerson has struck many as serenely disengaged from specific everyday concerns (and thus a free radical capable of bonding with any one of them) was that they had forgotten just how much his doctrine of the supreme *self* was devised precisely so that it could be mobilized for real-world action, which in Emerson's own case meant his long and gutsy rhetorical fight against slavery. In his newest book, Gougeon extends that argument by examining the wellsprings of the Emersonian dialectic of individual and society, from the beginning of his career to its end. How, in essence, does an individual's self-discovery become part and particle (to borrow Emerson's own phraseology) of the larger project of collective progress and happiness? Once again, Gougeon places abolition, the central social issue of the time, at the forefront of Emerson's exploration of the self, right from the early addresses and *Nature* (1836) through to the later works, such as *The Conduct of Life* (1860) and *English Traits* (1856).

The theoretical gambit is Gougeon's contention that there is a familial link between Emerson's theory of the individual and the psychomythic humanism of mid-twentieth century, specifically the work of Joseph Campbell, Erich Neumann, Norman Brown, and Mircea Eliade. The psychomythic approach claims, following the work of Sigmund Freud and particularly Carl Gustav Jung, that there are common themes in all human cultures, narratives that provide meaningful patterns and motives for individual and social development. In the West, the rise of Enlightenment science and philosophy had served to fragment any sense of humanity's common soul or collective unconscious. The psychomythic humanists attempted to put us back on our shared footing by reasserting the primacy of these archetypal patterns. Perhaps the most well-known was Campbell's hero monomyth, in which an individual undergoes a process of social separation, initiation into mystery, and a triumphant return to the collective bearing information vital to its regeneration. The monomyth signals the need for both individual and society to reintegrate its conscious and unconscious lives, to connect reason with intuition. On that score, Gougeon argues that Emerson's perennial fixation with the Eros (understood by Emerson as synonymous with Reason, God, and crucially the "Over-Soul") is the dynamic that produced Transcendentalism, which is ultimately a mode of being designed to break the hoary rule of the New England (and indeed, Enlightenment) "Fathers." Eros, the primordial Greek god of love and lust, styled as Cupid by the Romans, was for Emerson "the essential cosmic force, the glue that holds the universe and humanity together" (p. 7). The cultural hero answers his call by showing us how we too may align our intellect and our heart; Emerson models this kind of heroic self-recovery both through his writings and through his own example.

Despite its importance to Gougeon's argument, psychomythic humanism is the most vexing aspect of this book. On the one hand, we have no difficulty accepting Gougeon's point that the hero tale, for example, is a ubiquitous and handy genre for shaping lived and imagined content; on the other hand, his quick and dirty review of current cognitive science support for the theory of the unconscious falls a bit flat. I would only say that the existence or non-existence of the collective unconscious seems an unnecessary detail: it is quite enough to acknowledge that it is a powerfully useful metaphor.

This book is a stirring reminder of Emerson's continuing relevance to American life and letters. More than that, Gougeon shows us that Emerson truly was the archetype of the so-called "public intellectual," someone who can speak equally to matters of the heart and of the mind by demonstrating that, in the end, they are one and the same. Emerson was able to achieve a durable synthesis of reason and intuition because he underwent the hard, painful self-work that drew principle from lived discovery and experience. His philosophy, in other words, was first tested on his own pulse. How unlike so many of today's Brahmins and pundits, whose principles of duty and sacrifice are left for others to prove. One wonders how many of them will be on the right side of history, whether the issue is human rights, war, or environment. By contrast, Gougeon verifies that Emerson was indeed a Representative Man, of the type that can only be called hero.

Andrew McMurry
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Carla Yanni. *The Architecture of Madness: Insane Asylums in the United States*. (Architecture, Landscape, and American Culture.) Minneapolis: University of Minnesota Press. 2007. Pp. xi, 191. Cloth $82.50, paper $27.50.

The construction of hundreds of insane asylums during the nineteenth century in the United States was based on the romantic assumption that the proper institu-

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tional arrangements would facilitate cures. Enlightenment humanitarianism influenced the idea of kindly treatment of the mentally disordered and a faith in the malleability of human nature. In particular, John Locke's *Essay Concerning Human Understanding* (1689) set the tone for environmental determinism. Remove the afflicted as early as possible from their difficult home circumstances, place them in the benevolent arrangements of the asylum, and cure might be anticipated. Over time, this optimistic faith in institutional treatment proved illusory. The often chronic condition of insanity made asylums larger and more custodial. By the end of the century, a vanguard of neurologists criticized asylum medicine as antiquated, and the expense of erecting ever expansive structures raised questions about the best use of public funds.

In a well-wrought book, Carla Yanni provides a persuasive overview of the age of the insane asylum. As the author puts it, "My research on architecture does not undercut or radically alter recent conclusions in the history of psychiatry, but rather enriches the discourse through sustained and detailed study of changes to this building type over time" (p. 7). Indeed, following the balanced and nuanced work of scholars such as Gerald Grob and the late Roy Porter, she states, "My position is aligned with those historians who view the plight of the insane as serious, and who concede that asylums, while deeply flawed, were an improvement over dark, wet cellars, holes in the ground, or cages" (p. 10). As a specialist in art history, Yanni's major contribution is informing the discussion through the integration of abundant photographs and illustrations of buildings, floor plans, and landscapes into a clearly written text.

The book starts with the transition from the eighteenth-century madhouse to the reforms of the English Quaker William Tuke and the French doctor Philippe Pinel, who independently pioneered what became known as moral treatment: that is, a benevolent approach to care giving in which paternalistic physicians guided patients to self-control. The assumption was that this perennial scourge of humanity was not only treatable, but curable. Tuke's Retreat near York, for example, was purposefully separated from society in the country, a therapeutic "retreat." The design of the small-scale, linear building with a central edifice and two story wings on either side allowed for the classification of patients and an orderly regimen. The violent were separated under secure conditions. Restraint was used only as necessary, a practice made famous when Pinel dramatically unchained lunatics in Paris. In contrast to the standard armamentarium of mercury pills and bloodletting that were characteristic of Benjamin Rush's practice in late eighteenth-century Philadelphia, such Herculean interventions were kept to a minimum. Moral treatment became the ideal in new American institutions, such as the Friends Asylum at Philadelphia and the Hartford Retreat, during the early republic.

Thomas Kirkbride, superintendent of the Pennsylvania Hospital for the Insane, established the blueprint for moral treatment with his *On the Construction, Organization, and General Arrangements of Hospitals for the Insane with Some Remarks on Insanity and Its Treatment* (1854). This linear plan was a congregate type with connected pavilions laid out in a shallow V pattern. Work, recreation, and the landscape were incorporated into the architectural arrangements. The paradoxical effort was made to create a home-like atmosphere (e.g., with the use of parlors) in an institutional setting. The Kirkbride plan for hospitals was given the imprimatur of the powerful Association of Medical Superintendents of American Institutions for the Insane. Cultural issues also affected the living situation. Not only were the sexes separated, but at the Alabama Insane Hospital African Americans lived in the basement and whites in the upper tier, reflecting the racial hierarchy of the day. After mid-century, failure to realize the earlier utopian goals resulted in modification of the Kirkbride template. Separate institutions were created for the chronically ill, paupers, well-to-do, and criminally insane. Institutional critics, such as John Galt in Virginia, impressed by the freedom the insane had in the village of Gheel, Belgium, urged the decentralization of the linear plan. Detached cottages and farm settings were created for mental patients.

Nonetheless the size of public institutions expanded after the Civil War. H. H. Richardson designed a massive stone structure for the Buffalo State Hospital. Frederick Law Olmsted created elegant pastoral designs, not only for Central Park but for asylums. The elaborate edifices with large numbers of patients made clear that regimentation had replaced moral treatment. Yanni concludes with an instructive discussion of the changing spaces of mental illness from past to present.

LAWRENCE B. GOODHEART
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DAVID R. MEYER. *Networked Machinists: High-Technology Industries in Antebellum America*. (Johns Hopkins Studies in the History of Technology.) Baltimore: Johns Hopkins University Press. 2006. Pp. x, 311. $49.95.

In the history of American industrialization, machinists often drop out of the picture. Too entrepreneurial and well paid to garner the attention of labor historians yet too mechanical and pragmatic for business historians, they are the equivalent of non-commissioned officers in the military: supremely competent in the day-to-day affairs of the unit, natural leaders in a tactical sense, but rarely empowered enough to see the "big picture" issues of the overall campaign. Yet, as sociologist David R. Meyer demonstrates, machinists played a critical role in diffusing technological knowledge across the antebellum northeastern United States. In a very competitive business atmosphere, machinists collaborated to spread knowledge of new manufacturing methods. They read technical literature to learn new techniques, but Meyer argues that "tacit exchanges of component knowledge with other machinists while observing and working on actual machines" was essential to the trans-

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fer of knowledge (p. 16). The impact of such exchanges forms the core of this intriguing study.

Although it highlights new actors, in many ways the book follows the traditional narrative of industrial development. Meyer begins, for example, by looking at practices in the iron and textile industries in the late eighteenth and early nineteenth centuries. Turn-of-the-century iron foundries proved essential in improving upon steam engine designs, and Samuel Slater's adoption of British textile manufacturing techniques made Providence, Rhode Island, into a critical hub for early American machinists. Over time, the iron foundries and machinery works that serviced the initial wave of American textiles and small arms manufacturing proved to be excellent training grounds for a subsequent generation that built stationary steam engines, locomotives, and other essential tools of antebellum industrialization. Locomotive firms in the Boston, New York, and Philadelphia areas not only dominated the national market, but they also served as critical training grounds for railroad machinists. Textile firms were likewise dependent on four "nests" of firms that served the technical needs of their industry. Meyer is careful to reconstruct the geography of these networks. Each hub is critical to explaining the ways industrial practice spread across the growing nation, and each axis of production also contributed to the concentration of industrial innovation and production in the American Northeast.

Drawing inspiration from the high tech job markets of recent years, Meyer deftly recreates the fluidity of machinist networks as he painstakingly retraces their routes across the northeastern manufacturing corridor. Job-hopping from one high paying position to another reflected the machinists' substantial talents and allowed specific knowledge about new techniques in areas like locomotive manufacture to travel with them. "A large number of talented machinists created technical innovations in locomotive design and components," Meyer argues, "and the networks of know-how trading constituted the conduits through which to transmit the innovations" (p. 171). Machinists also facilitated the transfer of skills from one industry to another, as a familiarity with lathes, drills, and planers could easily translate into several different career paths. Finding patterns of interconnectivity is a common theme in Meyer's past work on American manufacturing, as is the focus on the American Northeast as the epicenter of America's industrial revolution. Recent scholarship in the field has questioned the absolute primacy of this region, seeking to highlight small-scale industrial development in areas like the American South. Rather than question this trend, Meyer reinforces the role of the Northeast as prime mover, while also explaining how expertise and techniques could move to outlying regions so quickly and effectively.

As a study with a regional focus but national implications, Meyer's book should prove invaluable to scholars of early American industrialization, and particularly to historians of technology. Those coming from the social or cultural side of the discipline might find it a challenging read. This is a methodical work that charges through several industries and spares no detail; in the process Meyer's machinists come across as a bit one-dimensional. The book's final chapter, for example, reveals that antebellum machinists came from the upper income brackets as well as from the lower reaches of society. A fuller discussion of how such contrasting backgrounds influenced the construction of machinist networks might have made the book more accessible to non-specialists. This is a small quibble, however, as the author's real intention is not to reconstruct the social world of antebellum machinists but to place their professional connections at the center of technology transfer in several critical industries. This book offers a convincing look at how the fluidity of "high tech" workers in the antebellum period influenced both the timing and location of early industrial innovation in the United States.

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William E. Wiethoff. *Crafting the Overseer's Image.* (Studies in Rhetoric/Communication.) Columbia: University of South Carolina Press. 2006. Pp. xxii, 234. $39.95.

While yeomen and small slaveholders have commanded their share of scholarship in recent years, the last thorough account of overseers is over four decades old (William K. Scarborough, *The Overseer: Plantation Management in the Old South*[1966]). As a study of the rhetoric through which the overseer's image was constructed, this book by William E. Wiethoff does not seek to provide a definitive study of this shadowy figure. The author does not systematically explore questions of interest to most historians of the Old South, such as the relationship between whites and the enslaved or the capitalist and/or modern nature of plantation economies. Nevertheless, his book is an interesting, accessible, and informative study of the processes by which the public, professional, and personal images of the overseer emerged.

Wiethoff's book will be of interest to historians principally because he seeks to discover the degree to which the overseer's image conformed to reality. Concerning the first image treated, the Taskmaster, Wiethoff finds quite a lot of convergence. Looking at overseers state by state (a pattern followed in every chapter), Wiethoff finds that they largely deserved their reputations as hard-driving, violent men with their eyes on the bottom line. But the overlap between image and reality is fuzzier in the overseer's other incarnations. The image of the Scoundrel, for example, owed as much to overseers' actual sexual and ethical misdeeds as to planters' prejudices toward ordinary whites. Behaviors giving rise to the images of the Scoundrel and Taskmaster produced a hitherto unappreciated image, the Rival. Competing with slaves for work and respect, overseers had ample reasons to drive slaves mercilessly. Professionally, the overseer's image bobbed between those of the Subal-

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tern and the Colleague. Class prejudices, the nature of overseers' work, and their association with slaves fueled the first image; overseers' contributions to their employers' wealth, as well as the family relationships that often bound overseer to employer, fed the second. This contrast presents Wiethoff with an opportunity to contribute to ongoing arguments about social class relationships in the antebellum South, but he does not engage them. That is a pity, because his evidence points to real unease with subordination and social mobility among slaveowners wealthy enough to engage overseers.

Significant tension is also in evidence concerning the two features of the overseer's public image Wiethoff identifies: the Spy and the Warrior. Law and custom in the South compelled overseers to monitor slaves closely after working hours. But Spies' reputations also suffered in planters' eyes. Slaveowners had mixed feelings about slave patrols because in them overseers served the public rather than their employers. Moreover, patrols' penchant for violence threatened the value of planters' property. So the Spy earned the enmity and contempt of master and slave alike. Overseers might have overcome this image by performing their final role, that of the Warrior. In peacetime, overseers were expected to join other whites in serving in the local militia. During the Civil War, overseers served a role similarly full of potential for rehabilitating their image: as the plantation Warrior, serving the Confederacy by maintaining discipline on the home front. But both forms of service further diminished overseers' image. Except in times of distress, few whites held the militia in anything but contempt. It was identified more with drunkenness, military pretension, and male camaraderie than the public safety. During the Civil War, overseers' service maintaining discipline and production on the plantation made them seem elitist and cowardly—associations that dogged planters generally during the war and eroded support for the war on the Confederate home front. In so many ways, overseers found themselves trapped between competing expectations. They could not win.

As the preceding discussion should make clear, this book asks a number of questions not typically posed by historians. The results are both surprising and frustrating. There are real payoffs here. Wiethoff's discussion of the Rival, for example, is fresh and original. Historians like Michelle Gillespie have traced the ways that skilled slaves competed with free laborers in southern trades, but seldom has it been recognized that this tension extended so far up the line as plantation management. In other ways, however, this book is not as useful as it might have been. Wiethoff's typologies are crude constructions. Although he takes pains to distinguish among southern regions (he finds interesting discrepancies in the territories carved from the Louisiana Purchase, for example), he collapses chronology, so that observations culled from seventeenth-century evidence are used to illustrate late antebellum developments. Wiethoff's study illustrates the maxim that idiosyncrasy entails both risk and reward. Historians of the Old South should approach it—with caution.

DANIEL KILBRIDE
John Carroll University

DANIEL KILBRIDE. *An American Aristocracy: Southern Planters in Antebellum Philadelphia*. Columbia: University of South Carolina Press. 2006. Pp x, 216. $34.95.

In this short but wide-ranging monograph, Daniel Kilbride explores the life styles, institutions, culture, and politics of southern, predominantly South Carolinian, planters in antebellum Philadelphia. He argues that a conservative, anti-democratic mentality, in addition to social and familial ties, united Carolinians such as the Izards and Middletons to local upper-class families like the Fishers. These southern and northern aristocrats, most of them Federalists, shared conservative politics and social conviviality. The latter were decidedly pro-slavery and pro-southern in orientation. Kilbride adeptly recreates the world of these American aristocrats in minute details before the Civil War disrupted their lives and interregional connections. Unlike Philip S. Foner's classic, *Business and Slavery: The New York Merchants and the Irrepressible Conflict* (1941), which traced the economic connections between southern planters and New York merchants, he is more interested in the social life and political and cultural values that created an elite class across sectional lines and set its members apart from the rest of American society.

Kilbride studies several Philadelphia institutions inhabited and supported by this class. Young women attended French academies that acted as finishing schools to prepare them for their lives as genteel southern ladies, well versed in French, music, and literature. They also imbibed the aristocratic ethos of their schools and disdain for middle-class values as well as for the masses. Young men attended Philadelphia's well-known medical schools and managed to acquire an education despite spending considerable time gambling, whoring, and imbibing the pleasures of street life. In an interesting statistical look at the student bodies of these schools, Kilbride reveals how heavily they were dominated by southerners. The American Philosophical Society provided an intellectual and political haven for some of the older men. The society, according to the author, not only promoted scientific and intellectual pursuits but became a center of upper-class male sociability. Many southern planters who did not live in the city traveled to it for short periods. Tourist travel to Philadelphia further cemented ties between planters and a national elite culture embodied in many of the city's institutions.

While Kilbride certainly provides us with a vivid portrait of upper-class life in antebellum Philadelphia and its many educational and scientific institutions, he tends to flatten the political differences between the people he discusses. One wonders how the political divisions between the unionists Petigrus and Draytons and the more sectionalist-minded Butlers played out in ante-

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bellum Philadelphia. Kilbride's notion of a united national elite political culture works much better for the early nineteenth century or the age of Federalism. But even here one should be cautious to note the difference between anti-slavery men like the Philadelphia doctor Benjamin Rush and rabidly pro-slavery Carolinian planters such as Charles Cotesworth Pinckney. In fact, this book tells us little about patronage and other connections between northern Federalists and free African Americans and, except for an allusion to Joseph Wilson's *Sketches of the Higher Classes of Colored Society in Philadelphia* (1841) at the end, reveals nothing at all about Philadelphia's prominent, wealthy African Americans. At least one of them, Robert Purvis, son-in-law of the sail maker and abolitionist James Forten, was the son of a wealthy South Carolinian and inherited his fortune. While Kilbride recounts the delight of southerners at anti-abolitionist mobs and their disdain for interracial mingling in the city, he does not spend much time analyzing their reactions to an active free black community and the long antislavery Quaker tradition, which made the city a hub of the Underground Railroad and abolitionist organization. Nor does he tell us much of the ire of southerners who regularly lost slaves in their travels to the north and were confronted at times by hostile and unsympathetic local police and law officials.

According to Kilbride, sectional tensions among this class of aristocrats surfaced only at the end of this period and with the outbreak of the Civil War. But even here he tends to downplay the famous "secession" of southern medical students from the city in the aftermath of John Brown's raid. The author is certainly right to portray the upper class as the most pro-slavery and conservative group in northern society, but in the end northern conservatives put the Union before slavery while their southern counterparts put slavery before the Union. Surely signs of this were evident well before the Civil War, as Kilbride himself shows in recounting the history and activities of the American Philosophical Society. There was no place for southern "firebrands" in this bastion of scientific inquiry and social elitism.

In short, Kilbride's interpretation works much better for the earlier part of the nineteenth century than for the late antebellum period. One wishes that he had examined the unraveling of a national, conservative, Federalist political culture with as much insight as he describes its making and functioning in antebellum Philadelphia. Nevertheless, Kilbride's book is a useful corrective to recent historical literature that emphasizes the allegedly middle-class and bourgeois nature of antebellum southern society and cities.

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SYLVIANE A. DIOUF. *Dreams of Africa in Alabama: The Slave Ship* Clotilda *and the Story of the Last Africans Brought to America*. New York: Oxford University Press. 2007. Pp. x, 340. $30.00.

This book does not make grand historiographic points; it rather delivers an effective micro-study of a revealing episode buried under successive levels of popular distortion. Sylviane Diouf examines what she demonstrates to have been the last illegal slave voyage, that of the *Clotilda*, which arrived in Alabama in 1860. As the author contends, this is "probably the best documented story of the American slave trade" (p. 249), particularly from the perspective of the African participants, who became objects of local pride and national publicity. The wealth of evidence offers a rich source on the human reality of the trade.

The episode had its origin in the proslavery enthusiasm of the late 1850s, and the calculation on the part of Alabama's Meaher brothers that, given the political climate, violators of the slave trade ban were unlikely to suffer much punishment. They read the political situation accurately, winning sympathy as hard-drinking scofflaws thumbing their noses at the Yankees. The Meahers, and their hired captain, occasionally boasted of their exploits, leaving a trail of evidence. The author emphasizes profit as their motivation, and with slaves selling on the African coast for ten percent of their American value, this piracy should have been lucrative, especially since few of the 110 Africans died en route. But the experience seems to have been a taxing one; the sailors had not been informed of the illicit nature of the cargo, and they struck repeatedly. The harassed owners scattered Africans in swamps to evade Federal pursuit, and only the outbreak of war broke up a serious attempt at prosecution.

Diouf combines managerial descriptions of the voyage with a wealth of survivor recollections to train a searchlight on the illegal trade. At this time, the king of Dahomey still made a lucrative enterprise of raiding inland ethnic groups. Diouf found that the *Clotilda*'s diverse human cargo consisted mostly of war captives from the interior of modern Benin and western Nigeria. This contrasts starkly with contemporary descriptions of the Africans as originating from a nonexistent "Tarkar" tribe. As an American historian, I am not well equipped to evaluate the author's interpretation of the African record. Still, Diouf demonstrates that the victims remembered much about their homeland, and her speculation on their origins sounds plausible. This is her real contribution: providing a modern sifting of the often-misinterpreted survivor evidence. At times the story rises to pathos, as with the chance encounter of some of the slaves with a circus; the familiar cry of an elephant filled them with excitement, in the hope that somehow home was near.

One central interpretive emphasis is that the African migrants encountered a mixed reception in the slave quarters; African Americans viewed them as uncivilized, heathen, even cannibals. The ritual scarification and filed teeth of several of the newcomers makes this interpretation of their response plausible, although direct evidence is limited. The hostile reception may explain the tendency toward endogamous marriage within the *Clotilda* cohort, and the insularity of the group even

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after freedom came. A dozen or so settled in an Africatown outside Mobile, a community that prided itself on retaining traditional ways well into the twentieth century.

Diouf's delineation of the postwar experience of this community is less sure handed than the earlier portions of the book. The author's tale shrinks to the Africatown cohort, understandably, but without much acknowledgment that their experience should not have been representative. Furthermore, the author could have given more thought to the economic implications of settling three miles outside the city, far enough to limit access to urban jobs. Diouf depicts the wider black community as mildly unsympathetic; my own study of Reconstruction in Mobile suggests that the Africans were generally ignored. At least, this was the pattern until outside investigators like Zora Neale Hurston made Cudjo Lewis and his aging cohort the objects of local pride.

There are other issues of detail, such as a map whose scale suggests the coast of Benin was about four hundred feet long (after p. 116); Oxford University Press should have caught this. Such quibbles notwithstanding, this is a valuable book on a significant episode in need of a modern treatment. It is clearly rooted in far-flung research and knowledge of the diaspora literature, and I found the author's determination to bring this community's story to the present day commendable. I hope the book reaches a popular audience.

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WILLIAMJAMES HULL HOFFER. *To Enlarge the Machinery of Government: Congressional Debates and the Growth of the American State, 1858–1891.* (Reconfiguring American Political History.) Baltimore: Johns Hopkins University Press. 2007. Pp. xiv, 258. $55.00.

This erudite monograph seeks to explain how the U. S. government grew from a small, weak administrative body in the antebellum era into the large, powerful entity it became in the twentieth century. It focuses on the period of Republican Party dominance that began with the Lincoln administration and ran through the Harrison administration. Williamjames Hull Hoffer relies heavily on the *Congressional Globe* and *Congressional Record* for primary sources, and he supports them with plenty of secondary and tertiary sources.

Hoffer challenges the standard "Whiggish interpretation" (p. xiii) of U. S. history, which holds that, from 1790 to 1860, the two-party system divided Americans into "nationalist centralizers-big government men" (p. xii), meaning Federalists and Whigs, and small government states' righters, meaning Jeffersonians and Jacksonians. The former mostly became Republicans in the late 1850s, says this orthodox school of thought, and once they assumed control after southern secession, they immediately began to create a "leviathan" (p. 203). Hence, from then until the ascendance of Woodrow Wilson to the presidency, only a struggling minority of small-government, laissez-faire Democrats tried to restrain federal growth.

Hoffer contends that, to the contrary, no preconceived big-government philosophy guided Republican decisions during the Civil War, Reconstruction, and the Gilded Age. The party did not have a preplanned agenda; Republicans merely expanded the size of government and the scope of federal power one agency, bureau, or department at a time to address some specific need of the nation at that moment. The changes they wrought thus resulted from pragmatism and necessity rather than from a deep-seated political ideology stemming from the Hamiltonian broad construction of the Constitution. "They were not philosophers, only politicians groping their way toward solutions to their problems" (p. xi).

To support this provocative thesis, Hoffer examines several specific bills debated in Congress. Framing his study between the bookends of the Morrill Land Grant Act of 1862 (which started in 1858 as a House bill) and the Second Morrill Act of 1890, he slogs point by point and counterpoint through the House and Senate debates over the various versions of those bills and a few others in between. The creation of the Departments of Agriculture, Education, and Justice, the Freedmen's Bureau, the Bureau of Labor, the Interstate Commerce Commission, and the Pendleton Civil Service Act are examined, as are the failed Blair Education bill, and the Circuit Courts of Appeals Act (which falls outside the latter bookend in 1891). The addition of these various administrative bodies, regulatory agencies, and reform measures to the existing apparatus of the federal government, says Hoffer, marked a "second state" (p. vii) in American political history. It amounted to a half-way point on the road between the "first state" (p. viii), meaning the "small, lightly staffed, largely amateur, and highly politicized" (p. 199) government of the antebellum era, and the all-out tidal wave of federal power of the "Great State" (p. ix) that began with the Progressive era and included the New Deal.

This book has offsetting strengths and weaknesses. Among the latter is the fact that, while it is certainly inarguable that each of these new pieces of the federal mosaic was born of a perceived practical need, that does not mean there was no deep philosophical foundation under the GOP that made their creation possible. Pragmatism and constitutional ideology are not necessarily mutually exclusive. The preponderance of evidence that one finds beyond this book indicates that Republicans as a whole did not share the ideological aversion to big government that Democrats of this generation did, which at least allowed them to entertain the notion of expanding the administrative state in such ways. Democrats would not have done the same had they been in power and faced the same issues at that time. Thus, lack of a "conspiratorial plot" (p. xii) by the GOP to create big government does not prove absence of an underlying belief in using the general welfare clause to greater effect.

A second weakness is that the very concept of the

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"second state" seems dubious. It is the product of an ambitious attempt to recast American political history from a different angle, which is not a bad idea but will not convince all readers. Even so, it will provoke us all to think outside the box of the so-called Whiggish orthodoxy. Indeed, one of the book's strengths is that it forces contemplation about why the growth of the U. S. government occurred when and how it did. Another strength lies in Hoffer's methodology of analyzing congressional debates from a lawyer's point of view. His way of looking at the issues in question tends to be detailed, thorough, and sometimes refreshingly unconventional. His biographical sketches of the individual congressmen who performed in these Capitol Hill dramas are also extremely helpful in showing how they arrived at their respective positions on various bills.

This book, despite its brevity, is not light reading. It may be suitable for certain undergraduate and graduate courses, but it is ideally suited for constitutional scholars in history, political science, and law, rather than for students. Academic and law libraries should definitely add it to their collections.

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WILLIAM W. FREEHLING. *The Road to Disunion*. Volume 2, *Secessionists Triumphant, 1854–1861*. New York: Oxford University Press. 2007. Pp. xvi, 605. $35.00.

In this book William W. Freehling concludes his two-volume account of why the southern states seceded from the Union and thus helped precipitate the Civil War. Seventeen years have passed since the appearance of volume one, and in that time, writes Freehling, his analysis has not changed nearly as much as his approach to historical writing. One of his main goals in this book, he emphasizes, is to reach non-academics who love history but find academic prose boring and so have turned to non-specialists who excel at dramatic storytelling but often have only "superficial" knowledge of the period they are covering.

With this in mind, Freehling relegates all historiographical material and all historiographical arguments to the endnotes. He also peoples his narrative with a colorful cast of characters, beginning with William L. Yancey, the hotheaded secessionist who learned to hate the North as a small child at his mean-spirited Yankee stepfather's knee. But despite Freehling's efforts, he has not completely shed his academic past. He still writes more like a first-rate academic historian than like a popularizer. The book, while rich in content, is too windy to be a page-turner.

As for the content of the book, it is much as Freehling says. Little has changed in his overall analysis. He seems to have abandoned his old notion—one that he probably picked up from our common teacher, Charles G. Sellers, Jr.—that slaveholders experienced "guilt" about slavery. While some of the men he portrays in this book were undoubtedly "uneasy" about slavery, none seem to have felt even one iota of "guilt" about it. Freehling also now minimizes the role of the West. In the first volume of this work, the annexation of Texas was undoubtedly a major event. In this volume, Freehling contends that territorial issues had little impact on the secessionists. Far more important, he argues, was whom Abraham Lincoln might appoint to important posts in the South, especially in those regions of the South where secessionists feared slavery was dying.

Overall, then, specialists will find few surprises in this book. As in all Freehling's earlier work, the focus is on the divisions within the Old South. Not only was the Lower South often at odds with the Middle South, and the Border South, there were sharp divisions within each region. Pro-slavery zealots in New Orleans had one vision, their counterparts in Charleston another. And even after Lincoln's election, contends Freehling, South Carolinians were divided. The conventional picture of the state rushing headlong to secession, he argues, is wrong. Many of the leaders were hesitant, fearful that their state would stand alone as it had thirty years earlier during the nullification crisis. It took an "incredible coincidence," the completion of a railroad linking Savannah and Charleston and the pledge of Georgia celebrants that they too backed secession, to push these hesitant South Carolinians over the edge.

The strength of this book undoubtedly lies in its focus. By keeping his eye on southern disunity, Freehling manages to offer interesting insights even about such familiar topics as Bleeding Kansas, the Dred Scott decision, Hinton Helper's polemic, John Brown's raid, and the election of 1860. Throughout, the detail is rich. But at times, I suspect, Freehling pushes the division thesis too far. Despite his well-crafted argument about the "incredible coincidence" of the Savannah railroad enthusiasts being on hand to cheer the secessionists, it is still hard to believe that "moderates" in South Carolina had a ghost of a chance in 1860. After all, the vote to secede was unanimous, 169 to 0.

LEONARD L. RICHARDS, EMERITUS
University of Massachusetts, Amherst

RICHARD R. DUNCAN. *Beleaguered Winchester: A Virginia Community at War, 1861–1865*. Baton Rouge: Louisiana State University Press. 2007. Pp. xix, 380. $40.00.

Civil War historians have long appreciated the town of Winchester, Virginia, not only for its wartime experiences but also for the expansive records its citizens left behind. However, with numerous books on library shelves either directly or indirectly focusing on Winchester during the war, readers may wonder what Richard R. Duncan can add to the discussion. Duncan responds by offering an area study combining inherent military focus with the stresses the war placed on Winchester's society.

The abiding interest in Winchester is rooted in the fact that it changed hands so many times during the war.

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In such a volatile environment, military activity, however brief and terrible, was followed by much longer and more torturous periods of living under the watchful eye of an occupying army and suspicious neighbors. For the citizens of Winchester, a town with an early introduction to an invading force, the lines of loyalty defined in the wake of the first local fight would leave long-term marks on the character of individuals. Duncan's keen historical eye has revealed much in this regard. In *Lee's Endangered Left: The Civil War in Western Virginia, Spring of 1864* (1998), he took a similar tack in examining the broader Shenandoah Valley, but this new book delves much deeper in the social results of localized warfare.

Throughout the book, Duncan displays an impressive understanding of the convoluted concept of loyalty. Observing that most of Winchester's wartime residents were women whose husbands, brothers, and sons were elsewhere defending the Confederacy and its ideals, he notes the prevailing southern loyalty while also treating the seldom-considered Unionists who found periodic relief from oppression when friendly forces took the town. By examining loyalty in this most fertile of fields, Duncan is able to paint a picture that highlights both the clarity and complexity of the question while accurately placing such vague and often overlooked ideas as civil authority and pressured nationalism in a position of evaluation.

As readers will certainly be impressed with Duncan's treatment of social history, they will also appreciate his ability to write the military side of the story. In addition to the traditional narrative of advance and retreat, the more elusive elements of military history are well represented. Although the book is by no means an examination of irregular warfare, Duncan devotes ample space to the phenomenon and successfully conveys to the reader the motivations, techniques, and results of such tactics. Interestingly, the author finds a much broader story of partisan warfare in the valley region than most readers might expect. Not satisfied with bushwhacking the lone soldier or sympathizer, the guerrillas inhabiting the hills and hollows around Winchester commandeered trains, stole thousands of dollars, and destroyed property.

In reading Duncan's work, one finds two points of concern. First, while Duncan has produced a well-researched and smoothly written narrative, he seems to argue no broader intellectual point. Such criticism should not be taken as damning, since his skillful crafting of the text, through his balancing of the military with the social, produces a book that enables its readers to draw many independent conclusions. Second, he simply mentions too many names, which slows a reader's progress through several chapters. The first chapter is particularly cumbersome because most of the hundreds of names quickly become extraneous information. This problem persists although to a lesser degree throughout the book. Similarly, Duncan pays homage to the historians who have contributed to his understanding of the Civil War; however, he sometimes overdoes it by including names within the text that would be better inserted in the notes. A cursory examination of the index reveals at least thirty historians listed with nearly 100 named references within the text. Such shortcomings are not pervasive, but they do grab the reader's attention when Duncan attributes fairly obvious statements such as "Virginia Unionists . . . interpreted Lincoln's actions [the call for troops] as betrayal" to a specific historian. Had he rewritten many of the passages containing quotes and left many of the names to be revealed in the endnotes, the book would have been considerably more streamlined.

Regardless of the aforementioned concerns, Duncan has written a fine book. His combination of military and social history embodies the decades-old school of thought that many historians have called the "new military history," but that is only now beginning to take center stage. Duncan's narrative and style will make his book popular reading among hobbyists, while his analysis and approach will hold sway among academics. This volume will play an important part in the discussion of Civil War Winchester for years to come.

BRIAN D. MCKNIGHT
Angelo State University

GEORGE S. BURKHARDT. *Confederate Rage, Yankee Wrath: No Quarter in the Civil War.* Carbondale: Southern Illinois University Press. 2007. Pp. xiii, 338. $37.95.

Recent events around the world confirm that civil wars are vicious and bloody affairs leaving wounds that can fester and reopen for generations. The United States endured a particularly grueling civil war between 1861 and 1865, which resulted in 620,000 American military deaths—more than in any previous or subsequent conflict. Despite these harrowing statistics, white Americans tend to view their civil war as an ennobling experience. The issues that caused the worst bloodbath in American history and the problems that the slaughter left unresolved fail to register in public memory. Civil War buffs seem content to remember only that both sides were valiant, which leads to the comforting conclusion that both were right. Even many professional historians who study the period succumb to its mythic allure, tempering their language and choice of projects to avoid alienating enthusiasts who revel in idealized depictions of mass murder.

Students of the black experience in the American Civil War have long confronted the sordid side of the struggle that most Americans regard with such fondness. White southerners embraced secession and founded a separate nation in 1860–1861 to preserve human bondage. These Confederates generally dealt harshly with those blacks who challenged the distinctive social and economic order that slavery made possible. During the past generation, scholarly books and articles devoted to the U. S. Colored Troops have established that Confederate forces repeatedly murdered black Union soldiers who fell into their hands, including those who attempted to surrender and wounded men

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too weak to offer resistance. Curiously, these racial atrocities have usually been studied as regional phenomena, and this narrow perspective has kept such research from altering the fundamental image of the Civil War.

In a bold and authoritative work that should rock the field's historiography, George S. Burkhardt argues that summary executions of black men in blue were not isolated or random affairs but constituted a pervasive pattern reflecting the values that called the Confederate States of America into being. His book shines a light on the conflict's darkest corners and shatters the comforting delusion that it was a titanic gentlemen's quarrel over differences white Americans reconciled with dazzling grace after they finished butchering each other in record numbers.

Abraham Lincoln's Emancipation Proclamation and his administration's formation of 166 black regiments awakened fears rooted deep in the South's slave system. Jumping to the conclusion that the Yankees intended to unleash a horde of African savages to wage a campaign of extermination against Dixie, Confederates vowed to reply in kind, ignoring the so-called rules of civilized warfare whenever they met black soldiers and the white officers who volunteered to lead them. As Burkhardt easily shows, rebel troops frequently followed through on this threat from their earliest encounters with the U. S. Colored Troops. As the Union Army's black regiments proliferated, southern desperation intensified, which produced more massacres.

Although the Lincoln administration pledged to retaliate for the mistreatment and murder of the Union's black defenders, it never did. Northern politicians refused to take any action on behalf of African Americans for fear of endangering white prisoners in rebel hands. Federal field commanders often ignored evidence of Confederate war crimes to avoid having to acknowledge them and create a host of complications. Black soldiers sometimes refused quarter to rebel troops, but Union officers and white troops often succeeded in preventing or limiting such excesses.

Blacks were not the only Union soldiers to feel the brunt of redoubled Confederate fury. As the war entered its final year, Federal forces embraced a hard-war policy that targeted Confederate civilians by legitimizing the destruction of any private property of value to the enemy war effort. Southern rebels responded to this harsher strategy by routinely murdering white Federals engaged in raiding, foraging, looting, and burning the homes of civilians suspected of sheltering guerrillas and other irregulars.

This book should be essential reading for anyone who is tired of viewing the Civil War as a sanitized historical pageant and ready to explore it as the tragic social revolution that it actually was. Burkhardt presents a comprehensive analysis of the factors that drove Confederates to rebel and expose Americans to a war without mercy that will supplant my edited collection, *Black Flag over Dixie: Racial Atrocities and Reprisals in the Civil War* (2004), as the standard work on this troubling subject. The book offers an effective challenge to Mark Grimsley's interpretation in *The Hard Hand of War: Union Military Policy toward Southern Civilians, 1861–1865* (1995), which claims that the Civil War fell short of total war. Along with the equally devastating *While in the Hands of the Enemy: Military Prisons of the Civil War* by Charles W. Sanders, Jr. (2005), Burkhardt's study demonstrates that many Americans who participated in this struggle practiced total war.

Gregory J. W. Urwin
Temple University

James K. Hogue. *Uncivil War: Five New Orleans Street Battles and the Rise and Fall of Radical Reconstruction*. Baton Rouge: Louisiana State University Press. 2006. Pp. xii, 227. $45.00.

James K. Hogue begins his study with the thesis that the Civil War did not end with the surrender of General Robert E. Lee at Appomattox or General Richard Taylor at Shreveport. This collection of seven essays admirably shows that New Orleans has a strain of violence that is peculiar to itself. The heart and bulk of the book lies in the examination of five major incidents of violence, including the New Orleans riot of 1866, the 1872 street battle generated by two Republican factions, the Cabildo battle of March 1873, the Liberty Place battle of September 1874, and the coup d'état of April 1877. We have known for some time that Reconstructed New Orleans was violent, but how violent has not always been fully appreciated.

Hogue's study clearly demonstrated that the end of the Civil War did not bring peace as far as New Orleans was concerned. As the author shows, Reconstruction simply represented another way to continue the war for many southern whites. Hogue suggests new ways of thinking about violence. With the passing of the Civil War, southern whites gradually adapted their strategy to challenge the legitimacy of the Republican government and compel it to depend increasingly on federal troops to remain in power. Avoiding all direct confrontation with the federal army, southern whites resorted to a strategy that moved from vigilance groups to paramilitary organization and ended with a successful coup d'état. The book presents a fine analysis of the byzantine nature of Louisiana politics during Reconstruction and judiciously awards New Orleans its appropriate place in that complicated and confusing political situation.

Not only is this study well written and a delight to read, but it presents a new and well-documented view of the role of the various parties and groups involved in violence in New Orleans during Reconstruction. The author clearly describes the patterns of violence and gives a complex and richly documented account of the civil strife that struck the southern metropolis. Hogue's elegantly constructed arguments shed new light on the evolution of violence in reconstructed New Orleans and Louisiana. As a result, Hogue also helps the reader to understand more clearly the intensity of the conflict

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that underlined Reconstruction in New Orleans and how the implementation of the Reconstruction policies degenerated into collective violence and street battles.

If on the whole, the analysis is quite satisfying, the study is not without flaw. Hogue's choice of the battles studied could be debated. One may question the fact that some major incidents that occurred in May 1867 or in September and October have been left out. Still, this book is a nice piece of work as it stands. Students of Reconstruction will find it a source of stimulating insights about the origins and the complex nature of violence during Reconstruction. Hogue has produced a solid piece of historical literature.

GILLES VANDAL
Sherbrooke University

JAMES B. BENNETT. *Religion and the Rise of Jim Crow in New Orleans*. Princeton, N.J.: Princeton University Press. 2005. Pp. xiv, 305. $39.50.

In the religious realm, interracialism in New Orleans and elsewhere in the American South had real possibilities. The influx of northern white denominations and limited racial inclusiveness in some southern-based churches showed promise for reciprocal respect and equality between whites and blacks in the years following the Civil War. James B. Bennett tests these potentialities in his comparative study of Methodist Episcopal and Roman Catholic religious bodies in New Orleans, describing the forces that created and sustained efforts toward black advancement in both ecclesiastical and civic settings and also the forces that ultimately undermined these efforts. Black Methodist Episcopal leaders, for example, became conspicuous advocates for black civil rights and wanted the civic life of New Orleans to reflect the same integrated interactions and activities that seemed commonplace in the churches and schools belonging to Wesleyan whites and blacks in the Louisiana Annual Conference. The presence of whites and blacks in the same Methodist and Catholic denominations nourished the expectation that this reality might stand successfully against increased segregation and discrimination in the city's civic life.

As race relations deteriorated by the turn of the twentieth century, blacks in the Methodist Episcopal Church supported interracial alliances, especially along class lines, to maintain racial equality in every facet of New Orleans's political, economic, and social sectors. Although many Wesleyan blacks were grateful to various Methodist missionary initiatives for establishing them in the middle class, they demanded an equal place with their white peers and benefactors in both church and society. Hence, they tried to rename the Freedman's Aid Society to bury its attitude of white noblesse oblige and opposed any merger between their denomination and the Methodist Episcopal Church, South, a religious body that remained unapologetic for its complicity with slavery and its ongoing racial paternalism. They also struggled for the election of a black bishop with prerogatives and responsibilities equal to those of his white counterparts. They were pleased with the elevation of Isaiah Scott in 1904, but disappointed that his episcopacy was confined to Africa. In 1920, however, the election of two black bishops, with the same jurisdictional possibilities as their white colleagues, seemed to realize the interracialism that Methodist Episcopal blacks had espoused for the several decades since 1865.

New Orleans Catholics shared with Methodists an interracialism in which white, black, and mulatto interaction appeared to be affirmed by their church's institutional practices and seeming inclusiveness. A Creole elite with strong roots in the antebellum period, for example, served as stalwart members of parishes and various religious organizations. Unlike the Methodists, however, the lack of a coordinated national program to serve black Catholics allowed the New Orleans archdiocese to pursue less aggressive efforts to evangelize blacks and publicly to oppose racial injustice. Hence, significant growth of black parishes and persistent shortages in the availability of black priests were not priorities. Although there were mixed parishes, their existence failed to translate to initiatives to end racial inequalities either in church or in society. The founding in 1895 and 1909 of the first segregated Catholic parishes symbolized the end of pretended interracialism. A new archbishop, appointed in 1888, had defended the religious and civic interests of black Catholics, but he acquiesced to growing segregated patterns that legally assigned blacks to separate venues in both church and state. This pattern persisted in the opposition of succeeding archbishops to the assignment of black priests to serve and evangelize African Americans.

Bennett concludes that blacks in Methodist Episcopal and Catholic churches, while they preferred religious bodies that carried no racial designation, still confronted the same segregation in the larger society that African Americans in the historically black denominations encountered. The principal difference lay in Methodist Episcopal and Catholic adherence to the fiction of interracialism. The dichotomy between blacks in Methodist Episcopal and Catholic churches and their counterparts in the historically black religious bodies, however, can be exaggerated. The sharp distinctions that Bennett identifies ignore a wide range of institutional and organizational interactions among ministers and members in both types of denominations. Moreover, there were varying ties between those in largely white religious and philanthropic organizations and others in historically black denominations. The differences between them, while important, should not obscure the common religious and racial ground that they occupied. Hence, Bennett's example of the black identity of the African Methodist Episcopal Church as a "cause of (racial) divisiveness," as understood by blacks in the Methodist Episcopal Church, should be viewed mainly as competitive rhetoric rather than an actual social reality.

DENNIS C. DICKERSON
Vanderbilt University

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SUELLEN HOY. *Good Hearts: Catholic Sisters in Chicago's Past*. Urbana and Chicago: University of Illinois Press. 2006. Pp. xiv, 242. Cloth $50.00, paper $22.00.

Chicago Catholics are justly proud of their rich history, in which nuns and sisters have played a significant role. Yet in the next decades American women religious may virtually disappear from the Catholic Church due to increasing age and declining numbers, as unsung as they were throughout the past two centuries when they served as the underpaid labor force for the church's educational, nursing, and child care institutions. In this book Suellen Hoy focuses on the role of sisters in the city's life from the antebellum decades to the era of civil rights activism. The seven chapters cover familiar ground that underlay this century of flourishing activity by women: the demography of the Famine migrations that brought Irish Catholics to America in large numbers (chapter one); the predominance of sisters with working-class and immigrant backgrounds; and the general ignorance of the public about the lives of women religious, which led to periodic conflicts between Catholic institutions and "American" organizations. Despite the fact that Catholic orders like the Sisters of Mercy (chapter two) taught mostly Protestant children, for example, they were not immune to nativist prejudice and misunderstanding.

In the course of her research Hoy found no communities of sisters who were "submissive servants of the male clergy" (p. 5). Instead, following existing histories of American sisterhoods such as those of Carol K. Coburn and Martha Smith, Maureen Fitzgerald, and Mary Oates, Hoy characterizes the activities of Catholic women religious as heroic and empowering, although governed by different paradigms than would ultimately guide Progressive-era reformers such as the female founders of Hull House. Despite their limitations, Catholic sisters were effective in teaching and ministering among the very populations that Hull House types addressed from a physical and intellectual distance. Unlike the settlement house workers, nuns and sisters lived within the densely settled urban communities they served, often with the abandoned or delinquent girls in their care. Chapters three and four treat the Good Shepherd sisters, a French semi-cloistered order with mostly Irish membership in Chicago, whose vocation was the care of troubled girls and "fallen" women with histories of criminality, drug addiction, prostitution, or illegitimate children. To them the sisters preached "work and self-reliance, not marriage and dependence" (p. 61). The Good Shepherd Magdalen Asylum on the city's North Side endured numerous challenges: an attack on the construction site in 1859 that may have been arson, total destruction of the property by the Great Chicago Fire of 1871, and acrimonious debates in the following decades on the legality of the sisters' reception of tax revenues for their ministries.

In Chicago as elsewhere, Catholic Church rules governing convent life still observed the hierarchical split between the elite choir sisters and those lay sisters who performed the everyday convent chores of cooking, fund-raising, and housekeeping. In a democratic society these divisions came to seem counterproductive over time, while local events such as outbreaks of cholera and tuberculosis (a major killer in convents) and the Chicago Fire also took their toll on the sisterhoods.

An overriding theme of race relations places Catholic sisters at the heart of many defining moments in Chicago and national history. In the early 1900s the sisters tried to respond to the needs of southern blacks migrating to the Midwest's industrial core by teaching embroidery, fine sewing, millinery, and laundering skills to African Americans at their Chicago Industrial School for Girls. When the archbishop of Chicago arbitrarily moved the Good Shepherd School in 1911 to join a campus for boys near Des Plaines, Illinois, the sisters were obliged to make their own plan without support from the Catholic hierarchy or their white neighbors, leading to the founding of the Illinois Technical School for Colored Girls in the existing South Side building. Over time, changes in national trends in child welfare began to favor foster care rather than the boarding school model offered by the sisters. Pressure to place children in family homes rather than in a group setting undermined the Good Shepherd institutions, and the school closed in 1953.

By that time, Catholic sisters in several congregations were already proving themselves to be progressive on race issues, even prior to the emergence of an organized civil rights movement. Without being radicals or systematic thinkers about race, the women had simply proved their "good hearts" by serving as missionaries to black neighborhoods and teachers and builders of schools in black communities, well before national media gave the American public the astonishing images of Catholic sisters in full religious garb marching with Martin Luther King, Jr., and others from Selma to Montgomery in 1965. The final three chapters chronicle these missionary efforts and the crucial role of orders like the Sisters of the Blessed Sacrament and the Sisters of Loretto, whose school for black children had opened in 1906. Members of other religious congregations like the Daughters of Charity were arrested for demonstrating against racist policies, while other sisters drew parallels between themselves and African Americans because they, too were "segregated, ghettoed, and because leadership is not encouraged in their ranks" (p. 149). A century later, their selfless idealism continues to inspire.

PAULA KANE
University of Pittsburgh

RICHARD N. JULIANI. *Priest, Parish, and People: Saving the Faith in Philadelphia's "Little Italy."* Notre Dame, Ind.: University of Notre Dame Press. 2007. Pp. x, 395. $35.00.

This book began as a biography of Father Antonio Isoleri, an Italian-born Catholic priest who, unlike the

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vast majority of his confreres, left a rich trove of personal papers. These included poetry, sermon manuscripts, devotional writings, and an autobiography. But the projected biography soon evolved into a history of the south Philadelphia parish to which Isoleri came in 1870 and of which he was pastor for fifty-six years. As Richard N. Juliani discovered, it was impossible to separate Isoleri's personal history from that of the institution to which he devoted his life. The expanded focus also permitted Juliani to examine the social and religious experience of Isoleri's Italian parishioners. What was the nature of their encounter with Catholicism in the American context? What role did religion play in the creation of the ethnic community? Questions like these are hardly new, but since the literature on Italian American religion is still relatively thin, they are well worth asking in this context.

Juliani's book begins with the founding of Philadelphia's St. Mary Magdalen de Pazzi parish in 1852, when it was the only parish in the United States established specifically for Italians. There were not yet many Italians resident in Philadelphia, and Isoleri's predecessors depended heavily for financial support on Irish Catholics who lived in the vicinity of the church. The reluctance of Italians to support their parishes, which was rooted only in part in their poverty, continued to be a problem, not only at Isoleri's parish but at the twenty-some parishes eventually formed by and for Italian speakers in the Philadelphia Archdiocese. Isoleri worried about money throughout his career, despite building a church, school, and orphanage. When he died in 1932, his parish was already in decline, as Italians moved away from their area of first settlement in south Philadelphia. Italian ethnic parishes were probably shorter lived, on the whole, than their Polish or German counterparts, although every ethnic parish was ultimately a doomed proposition.

Juliani ends his narrative in the mid-1930s, shortly after Isoleri's death and the not-unrelated demise of a neighboring Italian parish, organized in 1898 for a burgeoning population of immigrants from the Italian south. (Isoleri, like most of his original parishioners, was born in Liguria.) He discusses regional divisions among Philadelphia's Italians, which persisted well beyond the immigrant generation, as well as devotional life at St. Mary Magdalen de Pazzi. Juliani's overriding interest, however, is Italian ethnoreligious identity, necessarily negotiated in the hostile context of an Irish-dominated church. Every bishop under whom Isoleri served thought in terms of the "Italian problem": the modal Italian immigrant, in their view, attended church infrequently, failed to support the parish or its school, and tended to anticlericalism if not political radicalism. As Juliani tells it, the bishops were simply incapable of respecting Italian culture in its peasant incarnation and especially the Italian way of being Catholic. If his analysis of the problem is sometimes excessively partisan, he offers plentiful examples of Irish American tone-deafness with regard to their Italian coreligionists.

The book rests on an impressive array of primary sources, but it is nonetheless characterized by a pervasive superficiality. Isoleri's story is its most fully realized aspect, and even here the author stops short. Why no content analysis of the priest's sermons, especially since quoted excerpts from several of them suggest that he was an arresting preacher? Why no sustained analysis of his devotional writings, which were evidently prolific? Nor has the author adequately contextualized the social history of Philadelphia's Italians, despite that city's unusually rich historiography. Indeed, he sometimes fails to reference even basic historical information. Juliani's chapter on the 1890s includes not a single reference to that decade's grim depression. Nor does he discuss the high rates of remigration that were characteristic of Italians, although one can hardly understand the difficulties of ethnic community-building without taking this into account.

The book does contain a wealth of local detail, much of it conveyed by means of long block quotes. One almost feels at times as if one is reading a sourcebook. Still, these sometimes repetitious accounts from period newspapers and Isoleri's own writings will prove valuable to historians of Philadelphia and the Italian American experience, who have ample reason to be grateful to the author. I was grateful myself for the introduction to Isoleri, a man remarkable not only for his achievements as an institution builder but for the breadth of his learning. How many of his clerical contemporaries, one wonders, were as widely read in classical and Renaissance literature? He apparently had a mystical streak as well, about which I would certainly have liked to learn more.

LESLIE WOODCOCK TENTLER
Catholic University of America

ADAM FAIRCLOUGH. *A Class of Their Own: Black Teachers in the Segregated South*. Cambridge, Mass.: Belknap Press of Harvard University Press. 2007. Pp. 533. $29.95.

Already a distinguished historian of civil rights and African American education in the South, Adam Fairclough makes another significant contribution to those fields with this comprehensive study of black educators from emancipation to desegregation. Fairclough uses the term "teachers" expansively; the ranks of his educators include teachers and principals at every size of private and public school in both urban and rural communities as well as faculty and administrators in higher education and teachers' associations. His account is consequently a complex, multidimensional treatment that places teachers at the heart of the long struggle for educational equity in the South.

Fairclough opens with Reconstruction and the emergence of a southern black teaching class comprised of those who had been free before the Civil War and those who had acquired literacy during enslavement, showing from the outset the complex weave of experiences and intentions that would characterize southern black education. Calling the creation of public schools for all

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children "the single most enduring achievement of Radical Reconstruction" (p. 50), Fairclough argues that black teachers and their schools would remain at the center of southern politics for the next century.

Succeeding chapters demonstrate how black teachers became a key leadership cadre in southern communities. After Reconstruction, white southerners' demand that white northern teachers leave the region met southern blacks' growing support for an all-black teaching force. Southern black educators fostered a positive racial identity for a generation born in freedom and asserted local black autonomy against northern and white-dominated missionary groups and religious denominations. These teachers, most of them women, faced intense scrutiny of their personal and professional lives from community members and school trustees, impossibly short terms and scarce materials for instruction as well as salaries, and the callous disregard of county superintendents. Their labors were rewarded with a renewed struggle over black education waged by virulently racist white Democrats on the one hand and a coalition of northern and southern white reformers on the other. The reformers won their campaign for (white) state control over expanded public school systems for blacks focused on industrial education. Meanwhile, a group of black educators Fairclough calls "the founders" maneuvered within the curricular battle lines and between the whites dispensing philanthropic, denominational, and public monies to create a new generation of private schools and colleges.

Teachers themselves return to center stage in chapters on women educators and teachers in city schools, for whom gender and caste created ambiguous, sometimes problematic relationships with other African American and white southerners. These chapters also serve as a transition to the latter period of Fairclough's study, the 1930s through the 1960s. His discussion of teachers' associations begins with them in the late nineteenth century but focuses on their dramatic expansion in the 1920s and beyond as vehicles for educators' growing professional identity and political activism. By the 1930s, organized black teachers found themselves in an ambivalent position with the National Association for the Advancement of Colored People (NAACP) in particular and the civil rights movement in general. Black teachers filed pay discrimination suits and taught their students to assert their rights; they also balked when asked to risk everything they had built up over the decades—schools, funding, the security of their own middle-class respectability. Although it is unclear here exactly how and why teachers lost their public image as professionals leading their communities toward equality in these decades, teachers clearly resisted the NAACP's legal strategy of claiming that segregation made black teachers and schools inescapably substandard and inferior. As feared, desegregation and integration cost black teachers and administrators dearly in jobs and professional stature; the resulting loss of black leadership in public schools, especially, was keenly felt both by educators and their communities.

Many components of Fairclough's narrative have already been the subjects of extensive scholarship, but his signal contribution is to weave together seemingly divergent strands of experience into a cohesive account of black education. An adroit selection of individual stories keeps the focus on the human experience of teachers, whether a New Orleans principal like Fannie C. Williams or a better-known figure such as Charlotte Hawkins Brown. Of particular importance is Fairclough's attention to private and denominational schools and colleges that, he argues, maintained some measure of autonomy for black teachers within the region's expanding educational systems. Fairclough shows teachers as heroic educational missionaries and community leaders whose status and prestige were intensified by the disfranchisement and limited economic opportunities that hobbled black southerners. He also shows how the debilitating context of white supremacy forced teachers to play the game of black dependency when dealing with white school authorities and philanthropists. Teachers were also constrained by community perceptions of class, gender, and the value and nature of education as well as their own foibles and self-interest. But above all, Fairclough reminds us, teachers steered their students, communities, and the South to a broader conception of human freedom and opportunity.

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DANIEL S. MARGOLIES. *Henry Watterson and the New South: The Politics of Empire, Free Trade, and Globalization*. (Topics in Kentucky History.) Lexington: University Press of Kentucky. 2006. Pp. xii, 340. $50.00.

Few American newspapers have had a more illustrious history than the Louisville *Courier-Journal*, which, at least until its acquisition by Gannett in 1986, was considered one of the most respected and influential media properties in the United States. The *Courier-Journal* is generally associated with the Binghams, the talented and colorful family that controlled the paper from 1918 to 1986, when it was sold to the Gannett chain. One member of the family in particular, George Barry Bingham, Sr., draws particular attention from historians, and deservedly so, for he grew the *Courier-Journal* into a large media empire that dominated the state of Kentucky and exercised considerable influence in other parts of the United States.

However important was Barry Bingham—and he was important indeed—another man, Henry Watterson, was perhaps equally influential in the history of the *Courier-Journal*. Although he is little remembered today, Watterson, who edited the paper between 1868 and 1918, was not only responsible for the newspaper's rise to national prominence, but was also one of the most well-known (and controversial) southerners of his day. In this new biography of Watterson, the first since Joseph Frazier Wall's *Henry Watterson: Reconstructed*

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Rebel (1956), Daniel S. Margolies brings Watterson to the attention of a new generation of scholars.

Born into a "wealthy and politically prominent" (p. 17) Tennessee family in 1840, Watterson, after a bit of formal schooling, entered the rough-and-tumble world of American journalism before the Civil War. Capitalizing on his financial and cultural capital—his father Harvey was a powerful Jacksonian Democrat—Watterson rose quickly through the journalistic ranks, editing several different papers before assuming the position of editor of the Louisville *Journal* shortly after the war's end. In 1868 the *Journal* merged with a rival paper in the city to form the Louisville *Courier-Journal*, which Watterson edited for over fifty years before giving way once the Bingham family acquired the paper in August 1918.

Watterson, or "Marse Henry," as he came to be known, demonstrated both remarkable editorial consistency and hard-to-match rhetorical flair over the course of this period. During his half-century stint as editor of the *Courier-Journal*, Watterson banged the drum, loudly and proudly, for the South, sounding three resonant themes over and over again: "Home Rule, Free Trade, Sound Money" (p. 12). For much of this period, moreover, he was considered the nation's leading "tariff man," editorializing endlessly and verbosely in his paper for tariff reform, by which he meant low tariffs for revenue purposes only.

It is difficult for the modern reader to understand the passions aroused by the tariff in the late nineteenth and early twentieth centuries, and no one in the United States was more passionate about this issue than Watterson. Indeed, to the editor of the *Courier-Journal*, the tariff was not only the single most important developmental issue for the South, but also central to the destiny of the United States. To simplify (slightly), Watterson believed that under a free-trade regime both the South and the nation as a whole would prosper, the South because of increased exports of both its tried-and-true agricultural staples and new industrial products, and other parts of the country because of the growth of exports of agricultural and/or manufacturing products of their own. Under such a regime, moreover, America's economic might would only grow over time because from the free flow of commodities and other articles of trade out of the United States would come not only useful imports but also inflows of much-needed capital. So important to Watterson were tariff reform, free trade, and market expansion that by the late 1890s, he increasingly embraced imperialism as well, in the belief that formal control over additional territory would reinforce the above themes, and, in so doing, bring additional benefits to the American economy.

At the end of the day, though, free trade and sound money—and market expansion and imperialism as well—were viewed by Watterson as means intended to bring about a greater end: the establishment of a solid economic foundation for "home rule" in the South. And his commitment to "home rule" helps to explain his stances regarding other important issues such as race relations (for a Democrat of that period he was something of a moderate) and sectional reconciliation (which he supported). Along with a cohort of influential southern editors—Henry Grady, Daniel Augustus Tompkins, Richard H. Edmonds, and the like—Watterson was, first and foremost, a regional booster, promoting a "New South" that would in Watterson's words "out-Yankee the Yankees" (p. 32). If southerners did not do so, at least in his lifetime, it was not for any lack of effort or rhetoric on Marse Henry's part.

Margolies tells Watterson's story in an engaging way. The author trained in diplomatic history at the University of Wisconsin, and it shows in the close connections he makes between domestic issues and American foreign policy. It should be noted in closing that although Margolies includes the word "globalization" in his subtitle, he neither defines the concept nor engages the vast literature on it, preferring instead to employ the word variously as shorthand for, complement to, or substitute for free trade, empire, or the open door.

PETER A. COCLANIS
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W. PAUL REEVE. *Making Space on the Western Frontier: Mormons, Miners, and Southern Paiutes*. Urbana and Chicago: University of Illinois Press. 2006. Pp. x, 231. $35.00.

W. Paul Reeve's fine book implies that small stories often contain big ideas. Reeve's big idea is that the making of space in the late nineteenth-century western United States resulted from the close interaction between the various peoples who lived in the area. This assertion is not unique to Reeve, of course, but his book demonstrates how a fine-grained historical analysis brings the idea to life.

Reeve tells of a small area straddling what is now the border between southeastern Nevada and southwestern Utah from about 1860 to 1900. Encroaching on the territory of the native Southern Paiutes were Mormons claiming agricultural land for their Great Basin Kingdom and American miners whose activities centered in the town of Pioche. Unsurprisingly, as Reeve details, the Paiutes suffered the most in the encounters and emerged at the bottom of a kind of cultural, economic, and political hierarchy. Miners, representatives of American ideologies of progress, benefited from the support of the federal government. Mormons, whom Reeve treats with partial effectiveness as a separate ethnicity, occupied a middle position.

Reeve charts and illustrates the distribution of power in the interactions between the three groups. He does so most directly in the third chapter, focusing on how the "Mormon Question" and the "Indian Problem" were dealt with in political terms. Yet Reeve's main interest is not so much formal politics as interactions between cultures and world views. On those issues Reeve's book is at once highly informative and insightful, but not quite fully satisfying. To be fair, the prob-

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lems may result from the nature of the sources more than anything else. Reeve carefully and deftly handles an impressively wide range of sources, yet they do not seem fully to support the largest points he wants to make.

At the heart of the book are chapters on how Southern Paiutes, Mormons, and American miners each attempted to establish their world views within the shared space. These chapters not only skillfully describe the characteristics, distinctiveness, and variety within each world view but also closely analyze how these world views (and associated practices) interacted. Mormons and Paiutes, Reeve suggests, viewed both their claims to and actions upon the land in spiritual/communal terms, although the details of those terms differed substantially. Even the miners, who came to southern Nevada with explicitly individualistic purposes, viewed their contribution to American progress almost as a religious mandate. Rejection of each other's cultural claims was thus virtually inevitable. But Reeve also keenly and carefully identifies situations and spaces in which more fruitful forms of interaction took place.

The structural separation of the three groups in successive chapters makes it difficult to see how the three cultures came together simultaneously. Aside from the second chapter, the book mostly tells stories of interaction between two groups at a time. The examples abundantly cover all two-group permutations, but there are few illustrations of what it meant to live in a space occupied by Southern Paiutes, Mormons, *and* miners. Reeve attempts to tease this point out analytically, but I ended up wanting more illustrative argument.

Another feature that left me not quite satisfied was the relative lack of attention given to the fundamental concepts of space and frontier. While Reeve provides a solid conceptualization of sacred space, recent ideas from cultural geography and other disciplines would have helped to ground the argument that meanings assigned to space invariably result from interaction and struggle. And since Reeve clearly possesses a critical understanding of "the frontier," one wonders why a concept such as "borderland" was virtually absent. Finally, some readers may also have trouble following the book's rhetorical structure. It is conceptual rather than narrative, despite an abundance of small stories and characters. Few meta-storylines or key characters exist provide to continuity.

But these are small quibbles. Reeve has written a very useful book. It contributes impressively to the trend of treating cultural groups as collections of empowered agents, regardless of their position within structures of power. It shows that encounters encompass much more than an imposition of one group's worldview onto relatively helpless others. It shows people's creativity in new situations—indeed, the pitfalls and possibilities involved in making space with others who differ. This book is a rewarding read and will be useful to anyone seeking to understand historical trends in American intercultural interaction. Scholars interested in Native Americans, Mormons, and miners will find it particularly important.

ETHAN YORGASON
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LOUIS S. WARREN. *Buffalo Bill's America: William Cody and the Wild West Show*. New York: Alfred A. Knopf. 2005. Pp. xvi, 652. $30.00.

In this bold, hefty, and free-wheeling book, Louis S. Warren proposes to update scholarship about William F. Cody and his Wild West show by interlinking Buffalo Bill, his entertainment, and America. According to Warren, previous biographers have failed because they have either relied on contemporaries' glowing accounts to "prove" the Cody legend, or they have tried to debunk his heroic status. Another noted shortcoming is that biographers and scholars of the show "have been loath to trace his tailoring of show attractions to suit his audience" (p. 217). Warren divides his book into three parts: Cody's early life, his career as showman, and his decline in the early twentieth century.

In part one, Warren presents Buffalo Bill's early life on the Plains as prologue to his career as Wild West showman. Organizing chapters around acts in the Wild West show, Warren traces those scenes to happenings in Cody's early life. For example, the "Pony Express" act reflected a youthful Cody deftly linking his name to this enterprise that he "intuitively" knew encapsulated attractive components of western mythology. Likewise, the "Attack on the Settler's Cabin" emphasized domesticity and grew from Cody's experiences as "victim and perpetrator" (p. 38) of violence in Bleeding Kansas. "The Buffalo Hunt" act originated with his experiences as sport hunter and his recognition of the symbolic significance of the bison. While young, Cody met Wild Bill Hickok and copied his western persona and his penchant for tall tales.

Part two explores the transformation of frontier melodrama from lowbrow to respectable entertainment, the development of the Wild West show, and its generally flattering depiction of frontier values and America's place in the world. Buffalo Bill ensured the success of the enterprise by recognizing the appeal of Indians in entertainment and adding horses to the outdoor arena. To broaden the appeal of his "Wild West," Cody extolled domesticity, and curried the notion that he symbolized the consummate western man who balanced simple frontier virtue and civilized refinement. Warren demonstrates that the script resonated with Americans, foreign audiences, and performers.

Part three studies the decline of Buffalo Bill and his show after the turn of the twentieth century. Age, a nasty divorce trial, failed money-making schemes, and America's changing view of progress contributed to Cody's downfall. Warren concludes that Buffalo Bill accomplished much, including catapulting western entertainment into a national identity, successfully symbolizing an America torn between venerating its "Wild West" past and embracing modernization, industrial-

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ization, and the future. Somehow he even provided a sense of identity and pride for marginalized people who formed his Congress of Rough Riders of the World.

There is much that is praiseworthy in this award-winning volume; notably, Warren's skillful use of the ideas and methodology of luminaries, such as Alan S. Taylor, James Cook, and Leonard Moses. Like several scholars before him, Warren convincingly demonstrates that the Wild West show encapsulated the real and imagined happenings in Buffalo Bill's life, that Cody understood the "arts of deception," and that a comprehensive view of the show must include the Native American perspective. Moreover, Warren adroitly explores why Cody embellished his accounts, and how all of this resonated with audiences. Because he postulates that the United States was "Buffalo Bill's America," the author explores myriad personalities and events in the United States and abroad.

There are weaknesses in the book. Many in the historical profession will raise eyebrows about Warren's frequent use of "intuition" to link Cody to his age. In his chapters "Wild West London" and "Wild West Europe," Warren fails to emphasize that much of the definition of "American" developed by contrasting the Old World to the New during the European tours. In this regard, the preponderance of the historical evidence demonstrates that Londoners enthusiastically celebrated Buffalo Bill's Wild West show, but two-thirds of Warren's chapter on London deals with negative reactions to the Americans, with a sizeable portion of that chapter devoted to comparing Buffalo Bill and Count Dracula. Tours of other European nations are slighted. Sometimes Warren practices his own "arts of deception." In one instance, Warren tells the reader what Paul Gauguin must have thought as he walked through the Wild West show camp, despite a lack of evidence that he actually did that. Warren's frequent jabs at fellow scholars in statements such as the one about predecessors being "loath to trace the tailoring of show attractions to suit his audience" (p. 217) are unnecessary, untrue, and unkind.

Overall, this is a thought-provoking book written the way the history of entertainment should be crafted: as a reflection of the parent culture.

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JOHN W. HEATON. *The Shoshone-Bannocks: Culture and Commerce at Fort Hall, 1870–1940*. Lawrence: University Press of Kansas. 2005. Pp. x, 340. $39.95.

Toward the end of his detailed case study of the Fort Hall Indian Reservation (home to the Shoshone-Bannock tribe), John W. Heaton concludes that the tribe's "experience was important because it revealed that Indians on the periphery of mainstream American society could develop market-based and culturally based strategies to counterbalance asymmetrical relations with core powers and avert complete incorporation as marginal peoples. . . . [As a result,] the traditional balance between Shoshone-Bannock individualism and communalism endured" (pp. 240–241). Heaton's study joins a growing list of studies that have come to similar conclusions: Indians did not, as the designers of late nineteenth and early twentieth-century assimilation policy had hoped, melt into the mainstream and cease to be Indians. From the end of the Civil War until the end of the New Deal, Native peoples adapted to changing conditions in myriad ways—some intentional, some not.

Their populations plummeted and they lost a lot of land. But they also became successful stock raisers and farmers and willingly embraced capitalism. Many became Christians, while others stuck to more indigenous forms of religious experience; many mixed faiths. Savvy individuals emerged as leaders to challenge non-Indian claims to land, gain a measure of economic independence, and craft a vision of Native sovereignty. Arguing that Indians adapted and responded to changing conditions is still, it seems, historiographically significant. It is unfortunate that scholars of Indian history feel compelled to argue against outmoded ways of seeing Indian history that argued otherwise. But the insistence on demonstrating that Indians did not disappear, that they creatively responded to massive change, and that they engaged in a market based economy while still maintaining an Indian identity—whatever that might mean from place to place and time to time—is warranted when modern Indian history remains almost entirely unknown to most Americanists.

Like historians such as Brian Hosmer and those collected in Hosmer and Colleen O'Neill's *Native Pathways: American Indian Culture and Economic Development in the Twentieth Century* (2004), Heaton's focus is on the intersection of economics and culture. Using an approach that he argues is a synthesis of cultural constructionism and historical materialism, Heaton is interested in exploring how Shoshone-Bannock culture influenced economic choices and in turn how those choices affected their culture. By looking carefully at the "dialectical relationship between external forces and internal influences" (p. 12). Heaton aims to show how the market influenced and was influenced by, for example, Shoshone-Bannock communalism. This had positive and negative effects. As Heaton shows, some Shoshone-Bannocks were able to achieve a significant measure of economic self-sufficiency (and others at least not total dependency) through cattle ranching and other pursuits and were also able, once powerful economic interests organized in the 1920s under the guise of the Fort Hall Indian Stockmen's Association, to control the political economy of the reservation.

But as Heaton wisely notes, what were in large part a series of shrewd responses to outside influences had a corroding effect on some aspects of tribal life. "Incorporation pressures generated by core powers on the Fort Hall periphery . . . served as ongoing sources of unity for Shoshone-Bannocks. Yet these same forces also compelled Shoshone-Bannocks to adapt and innovate economically, a process that increasingly divided

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the community in the early twentieth century. Thus, what brought the people of Fort Hall together also threatened to drive them apart" (p. 128).

Heaton's research into these matters is comprehensive. The extensive record keeping of the Bureau of Indian Affairs made it possible for him to document, at times in exhausting detail, the economic changes experienced by the Shoshone-Bannock from the middle of the nineteenth century until about 1940. He demonstrates conclusively that the market indeed affected Shoshone-Bannock culture and vice versa: an entrepreneurial spirit emerged, and religious practices like the Sun Dance revived, among other things. But Heaton does this early and often and a certain amount of repetition sets in. It would have been helpful to reduce the number of examples, streamline the story, and considerably shorten the first chapter on the pre-reservation period as it does not add much to the ultimate goals of the book. It would also have been wise to resist the impulse to discuss methodology and other historians' work in the text.

Heaton does an excellent job of further complicating the Indian response to the Indian Reorganization Act of 1934, making clear that the response to the revolutionary legislation varied across Indian country. He is also masterful at making this an Indian-centered story. Indian voices are not hard to find; they are everywhere. In sum, Heaton's book is a welcome addition to a growing body of literature—mostly focused on individual reservations—that demonstrates the dynamic Indian response to the pressures of assimilation and the market economy.

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COLL THRUSH. *Native Seattle: Histories from the Crossing-Over Place*. Foreword by WILLIAM CRONON. (Weyerhaeuser Environmental Books.) Seattle: University of Washington Press. 2007. Pp. xxi, 326. $28.95.

All too often urban public history neglects a Native American presence in describing the history of American cities. Indians appear early on when a site is being "discovered" and then they exit stage left. They may appear later as mascots for sports teams or as street names, but their actual history and their deep-seated knowledge of local places and local geography is lost. Coll Thrush and the Weyerhaeuser Environmental Books Series are about to change that. As William Cronon explains in his foreword, the nineteenth-century myth of the Vanishing Race of American Indians "has always been a cruel lie, distorting the actual lives and histories of people who remain fully present" (p. vii). Seattle has proudly proclaimed its Native heritage while paying little attention to the actual plight of tribal peoples within its urban boundaries. Through masterful work with census records, ethnohistories, oral histories, an understanding of Chinook jargon, and ancient Whushootseed terminology, Thrush has placed Indians back into the history of Seattle in the longhouses of Little-Crossing-Over Place, which is Seattle's indigenous name.

Thrush begins with an analysis of the 1850s speech attributed to Chief Seattle, which includes the line "and when the last red man shall have perished from the earth and his memory among the white men shall have become a myth, these shores will swarm with the invisible dead of my tribe." Whether a local leader of Duwamish and Suquamish heritage named Seeathl actually spoke those words is impossible to determine, but the speech is a critical eco-text and as metaphor it frames the book; Thrush's goal is to search out and resurrect a buried Native past. The author successfully recreates Seattle's first decades and the necessary blending of white pioneer settler enterprises with Native labor and traditions. But as the pioneers expanded into adjacent bays and estuaries, cutting trees and filling wetlands, they obliterated native places, destroyed ancient fisheries, and displaced any Indian residents who had not succumbed to smallpox.

Native peoples were pushed farther and farther away from a coastline that was theirs. Thrush describes these wrenching transformations, the racism, the suicides, the arson, the inevitable spirals down into wage labor and prostitution as Indians became Seattle's underclass and "homeless vagrants" in their own homeland. From the first wave of white pioneers more and more *cheechakos* or newcomers arrived, and by 1930 less than one percent of Seattle's population was Indian. "No Indians Allowed" signs permeated downtown despite the symbolism of a giant totem pole in Pioneer Place Park. Bars in the Skid Road area frequented by Native Americans were labeled "bow and arrow joints," and thousands of Native people, residents, and itinerant workers lived in aging hotels.

Thrush manages to locate threads of numerous native peoples as they pass through Seattle including Lummi, Swinomish, Puyallup, Haida, Cowlitz, Colville, Makah, S'Kallam, Yakama, Tlingit, Muckleshoots, Tulalip, and Snohomish. More Indians entered the urban area with World War II factory work, and by the late 1950s groups formed such as the American Indian Women's Service League and the United Indians of All Tribes. By the 1970s, in local "fish fights," tribes asserted treaty rights and gained new respect as ecological stewards of waterways and natural resources.

A superb ethno-atlas with regional maps in the appendix is an extensive reference to indigenous place names, but for context the book could have benefited from an analysis of evolving federal Indian policy in the Northwest simultaneous with Seattle's urban history. A map showing the location of modern Indian reservations on Puget Sound also would have been helpful.

Thrush argues that "the central problem of Seattle's Native pasts [is] the distance between the imagined Indian in the city and the real experiences of Indian people" (p. 200). Using standard historical sources, poetry, promotional pamphlets, pioneer reminiscences, native perspectives, and active tribal participation, Thrush has created a new template for studying American Indians

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in American cities. As an urban Indian palimpsest, by grounding Seattle and Puget Sound geography with Native names and by documenting the continuity of Native peoples over time and place, it succeeds as a benchmark book.

ANDREW GULLIFORD
Fort Lewis College

JUDY MATTIVI MORLEY. *Historic Preservation and the Imagined West: Albuquerque, Denver, and Seattle*. Lawrence: University Press of Kansas. 2006. Pp. x, 204. $29.95.

"Larimer Street in Denver was the most famous street in the West. The restaurants and hotels of its heyday were world renowned. . . . And tales of what went on behind closed doors in the neighborhood shocked a nation" (p. 55). Passages like this seem good candidates for the "what-not-to-do" list that historians give their students at term-paper time. Yet this particular fragment, drawn from a 1965 market analysis and repeated in numerous publications in the years that followed, laid the case for the development plan that arguably revived the largest city in the Rocky Mountain West.

Judy Mattivi Morley's study of historic preservation in Denver, Albuquerque, and Seattle comes filled with such examples of third-rate historical ideas that have supported first-rate urban redevelopment schemes in the late twentieth and early twenty-first-century West. As her subtitle suggests (and as her repeated insistence that "identity is a created concept" [p. 153] renders explicit), historic preservation practice has demanded a flexible and at times simply erroneous understanding of the past. To this end, she suggests that "[r]ather than evaluate a historic district compared to its historical antecedent, it is important to evaluate the district based on choices that planners, developers, and local officials made in the 1950s and 1960s"—the period in which most of the developments that comprise her study are rooted (p. 13).

The idea that preservation draws on a socially constructed past is not in itself new; the idea that that imaginative reconstruction appears especially clearly in the American West draws from a more specific line of works, including John M. Findlay's *Magic Lands: Western Cityscapes and American Culture after 1940* (1992), that have similarly excavated the idealization of western history in its urban context. But Morley's study still proves useful as a set of well-grounded, thoroughly researched case studies from which planners, planning historians, and western historians alike will draw lessons that, even if familiar, would but for this book be forced to rest upon far more anecdotal evidence.

Morley's research brings us face to face not only with the bricks and mortar of her five study sites (most of which will be only superficially familiar, even to historically minded readers, as sites of a dinner eaten or a souvenir purchased) but also with the characters whose dedication to preserving the urban fabric in the wake of postwar incentives for clearance and renewal—here as in other American cities—became the key element in reversing that trend for a few important urban neighborhoods. Denver's Dana Crawford and Seattle's Victor Steinbrueck may not have "shocked the nation" or even acquired a level of recognition approaching that of Molly Brown or John Nordstrom, but they and their ilk, as Morley demonstrates, accomplished tremendous change from an intensely personal level, garnering grass-roots support and goading city officials to gamble on sites dismissed by cautious real estate investors and city planners.

Preservation itself, in this nation, rests upon a paradox: to save the buildings of the past is to defy the logic of discard and rebuilding by which they appeared in the first place. Morley's studies refine that paradox with a particular eye on the history and mythologies of the West. Here is Albuquerque's Old Town, annexed—against the wishes of the area's primarily Hispanic population—by a white population intent on capitalizing on their city's heritage, and redecorated in a manner more befitting Santa Fe in the 1930s than Albuquerque in the 1890s. Here are Larimer Square and its Denver neighbor, LoDo, competing to attract tourists to the sites of deserted warehouses and residential hotels. Here are Pioneer Square and Pike Place Market in Seattle, a city trading on its populist image as it sweeps homeless residents and marginal retailers from its spruced-up waterfront.

How particular are these paradoxes to an "imagined West"? How distinct are the preserved districts of these three cities (and, by extension, Los Angeles's Olvera Street, San Francisco's Jackson Square, and any number of sites the author might have selected from the region) in the national narrative of urban preservation? On these matters, the reader receives minimal guidance. We learn little of the national impact of the 1949 and 1954 housing and renewal acts; of the subsequent turn to neighborhood-based community development grants; of the application here or elsewhere of the Tax Reform Act of 1976. Morley's book works best, in fact, in those passages where she brings a more journalistic approach to her subject, enlivening her sites' histories with generous portions of city council minutes, local color, and gossip. Her effort to apply a broader historical perspective to the work yields an impressive bibliography, filled with the work of authors ranging from Jean-Christophe Agnew to Robert Wiebe; it results in notes dutifully reminding us (not once but twice) to see Eric Hobsbawm "for more on invented traditions" (pp. 170, 180); but it reveals only enticing glimpses of the more concrete links from local to regional to national experience, from policy to practice, from image to reality, that the material still merits.

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CRAIG MINER. *Next Year Country: Dust to Dust in Western Kansas, 1890–1940*. Lawrence: University Press of Kansas. 2006. Pp. xx, 371. $34.95.

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Craig Miner's family is from western Kansas. This is the essential fact of Miner's latest book about this often misunderstood region of the American West. Drawn largely from western Kansas newspaper snippets and the opinions of their hopeful and often wistful editors, this volume brings the high plains to life. Divided both topically and chronologically, Miner begins with his childhood recollections of his grandfather William D. Miner: "I have the watch. I have the heritage. And the blood is only part of it" (p. 2). He then follows the Miners of Ness City and their counterparts in Liberal, Larned, Garden City, Hays, Dodge, and others, through five tumultuous decades, tragically ending with a drought. These were tough individuals who, the Hays editor noted, "take their beatings standing up and without flinching" (p. 289). Miner's stories take us back and forth from place to place, from settlement to the machine age and the development of towns as they were founded, prospered, nearly died, and sometimes were resurrected. Fanning the flames of hope for a better future were the small-town newspaper editors, whose own grit and determination to stay made them the self-appointed spokesmen for Next Year Country.

For historians of the American West, and particularly Kansas, the stories Miner tells are not atypical, but the freshness and immediacy of his descriptions of so many modest hopes and dreams begin to add up, drawing the sympathetic reader into the network of small town life. This is the crowning achievement of the book. So many factors were beyond the control of western Kansas farmers and businessmen—the climate, railroad connections, wheat prices, federal programs—that the pluck amply illustrated through Miner's retelling of small town news seems heroic. One cannot help admire some of Miner's subjects, like the Kinsley Shakespeare Company (called "Fairies in the Short Grass") or local enthusiasm for the first successful airplane flight at Larned in 1910, where a crowd of 3,000 "rushed forward and tore the airplane apart to take home parts as souvenirs" (p. 215).

There is tragedy, too. Accidents, isolation, and despair claimed the lives of countless residents in this unpredictable region, while all suffered economic hardship. Miner traces the fortunes of hardware merchant A. A. Doerr, whose thriving business failed on the shoals of low wheat prices and unrecoverable credit extended to farmers after World War I. Mail order catalogues, delivered unbidden by the train car load, further undermined local businesses. Town leaders darkly warned that "the future of the community hinges absolutely on community loyalty from every one of its citizens" (p. 215).

Miner's personal connections, deep knowledge of the region, and skill with a good story make it hard for him to be dispassionate. This is a book that celebrates western Kansas exceptionalism and by extension, Miner's role as the keeper of the story. Unsurprisingly, Miner takes exception with Thomas Frank's bestselling critique, *What's the Matter with Kansas? How Conservatism Won the Heart of America* (2004). Frank, we are reminded, is not from western Kansas, he is from Overland Park, a Kansas City suburb. Frank asked how people with a long history of progressive idealism had become a tool of the far right, particularly the monied and class conscious leadership of the Republican Party. Miner's answer is disarmingly simple: we never were progressive, we were never really Populists. It is true that political Populism was more of an eastern and central Kansas phenomenon, but much of Miner's own research makes it clear that farmers throughout the state were unhappy with their lot.

In trying to give western Kansans immunity from Frank's thesis, Miner instead makes them the victims of a single-crop economy that tightened and loosed without rhyme or reason. In conclusion, Miner tells us the residents of western Kansas were gamblers, independent, and maybe a little too optimistic. He becomes a booster himself in concluding that "the power of positive thinking, combined with self-discipline and realism, could indeed accomplish great things" (p. 278). Like James Malin of Kinsley before him, Miner's interpretation of the history of western Kansas is the story of heroic adaptation to a difficult environment. An "unusual climate in Western Kansas had attracted and bred unusual people" (p. 300). In the end, Miner criticizes standard interpretations of industrialized agriculture because they fail to give western Kansas its due as a unique story. Yes, it is a great country, but is it so different that we should give it particular place? If that was Miner's purpose, he will find a sympathetic if small audience highly appreciative of his efforts.

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Leslie Butler. *Critical Americans: Victorian Intellectuals and Transatlantic Liberal Reform*. Chapel Hill: University of North Carolina Press. 2007. Pp. xv, 381. Cloth $65.00, paper $24.95.

Leslie Butler's sympathetic examination of those New Englanders long dismissed as elitist "Mugwumps" recuperates an older tradition of intellectual activism by placing it an Anglo-American context. In that sense, the book fits into a larger project of de-nationalizing American history, especially the history of the late nineteenth and early twentieth centuries, as in the influential books by James T. Kloppenberg and Daniel T. Rodgers. One of Butler's important contributions is to move that internationalization farther back, before the Civil War, showing how some leading antislavery radicals, while moving into the Republican Party, also sought an alliance with Gladstonian liberalism in the years of the great reform bills and the later struggle for Irish Home Rule.

The book focuses on Thomas Wentworth Higginson, George William Curtis, James Russell Lowell, and Charles Eliot Norton, although many others (all men), both English and American, are recurring characters in this "community of liberal men of letters." From the 1840s into the new century, Butler's four subjects

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marked the spectrum of Victorian liberalism, from militant abolitionism in Higginson's case, to the founding of major cultural institutions, including the many museums and societies fostered by Norton, and the preeminent magazines associated with Lowell and Curtis, *Harper's Weekly* and *The North American Review* (with E. A. Godkin and *The Nation* also figuring considerably). Besides numerous personal affiliations, what linked them was politics, and a particular politics of culture. They were all abolitionists and then Republicans, deeply bound up with the party that emancipated the slaves and the Civil War as a "moment of heady idealism." Indeed, it is central to Butler's argument that these were serious partisans, although they were routinely baited as priggish, carping critics, especially when their common cause became civil service reform, as Republican "bolters" backing the upright conservative Democrat Grover Cleveland in 1884.

Shared experiences of partisan mobilization and a war of liberation, and their formation in Ralph Waldo Emerson's Boston, shaped a particular understanding of culture rooted in "liberal Protestantism," "self-culture," and self-improvement. All four (and many others like them in the world of magazines, publishing, and the new postwar universities) were committed to Arnoldian principles of "cultivation" in the service of democracy. At its most utopian, this led to proposals for mass dissemination of "dime Shakespeares" to reach the working classes; at its most practical, it was manifested in Lowell's and Curtis's influential magazine columns, which brought a vast transatlantic middle-class readership into a common conversation about literature, the arts, and politics.

Butler's cast is considerably larger, however, encompassing a wide range of British intellectuals and political figures, including James Bryce, Goldwin Smith, A. V. Dicey, John Morley, and Leslie Stephen. She also traces the profound influence of John Stuart Mill and Thomas Carlyle as intellectual avatars to a generation of young New England men in the 1840s and 1850s who were painfully aware of their creole provincialism and the absence of a serviceable American culture. Using their transatlantic correspondence, meetings, friendships, and periodic tensions over many decades, she shows how each side drew upon the other: the British for the vindication of the republican experiment in popular rule; the Americans for validation and intellectual guidance. Together they built "a transatlantic liberal synthesis" well before the rise of labor and social democratic parties in Europe and the Progressive movement in the United States.

Over time, Higginson, Lowell, Curtis, Eliot, and their cohort anchored "a new cultural establishment" in the United States that was "both worldly and staunchly nationalist." Yet as the century waned they were increasingly at odds with what they, like many liberal intellectuals since, saw as a grossly corrupted culture and a sham democracy. Their last hurrah became the Anti-Imperialist League, denouncing the brutal counterinsurgency war in the Philippines of the early 1900s. By that time they were aging men, prophets of a democratic past now indeed past.

This book vindicates the author's argument that bourgeois intellectuals are uniquely suited to bring the advantages of their learning to vastly larger masses. It also demonstrates that innovative practices of cultural democracy were made possible by the Anglo-American connection, at a fateful moment of rising American power. What remains unclear is why these men, in the forefront of radical republicanism at its mid-century high tide, went so completely into the political cul de sac of civil service "reform" for so long, oblivious to both the rise of Jim Crow and the new corporate capitalism. To begin with William Lloyd Garrison and Charles Sumner, and then end with Grover Cleveland, begs more explanation than the reader will find here. Is this the inevitable fate of men and women of letters who engage fully with the practical mess of politics: to be left behind as the world changes? One hopes not.

VAN GOSSE
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MICHAEL KAZIN. *A Godly Hero: The Life of William Jennings Bryan*. New York: Alfred A. Knopf. 2006. Pp. xxi, 374. $30.00.

During the 1896 Democratic national convention, William Jennings Bryan electrified the Chicago Coliseum with a stirring indictment of the financial establishment and its efforts to "crucify mankind upon a cross of gold." The assembled Democrats nominated the young Nebraskan as their presidential candidate, marking the shift in power within the party from the conservative East to the reform-minded South and West. Twenty-nine years later, Bryan served as prosecutor and witness at the Scopes "Monkey" Trial, seeking to strike a blow against the teaching of evolution. Bryan's legacy has been debated ever since. Had the Great Commoner crossed over to the side of ethnoreligious intolerance? Or had he kept the faith of progressive reform?

Michael Kazin weighs in with a masterful telling of Bryan's life. Whether battling the gold standard or Darwinism, Kazin argues, Bryan drew his faith from the mutually reinforcing combination of progressive politics and evangelical piety. Lawrence W. Levine made a similar case in his fine treatment of Bryan's later career (*Defender of the Faith: William Jennings Bryan, the Last Decade, 1915–1925* [1965]). But Kazin's purpose is more ambitious. He seeks not only to redeem Bryan, but to elevate him to the giant of reform who built the progressive foundations of the New Deal.

Bryan ran for president three times promising currency reform, the income tax, and other popular measures. But it was his booming voice that thrilled delegates at conventions and crowds at campaign stops. "Whatever he achieved," Kazin explains, "depended on the power and durability of his voice and the romantic tenor of his words" (p. 304). Kazin offers keen insights into Bryan's oratory and into the passions of those men and women who came to listen. After each convention

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triumph and each defeat at the polls, Kazin reproduces words from "Bryan's people": hundreds of thousands of often middle-class and law-abiding men and women who sent adoring letters to their crusading hero. Although only a fraction of the correspondence survives, the sentiments from the letters provide a chorus for Kazin's telling of what he calls "the longest-running personal melodrama in American politics" (p. 170).

After his last presidential run and a brief stint in Woodrow Wilson's cabinet, Bryan devoted his energies to a profitable career on the lecture circuit. He spoke against the evils of alcohol and war, but he mainly lectured about religion and the threats posed to faith in Scripture. Uncomfortable among the growing immigrant population in Nebraska, Bryan moved to Florida, a more congenial environment for his white supremacist and sectarian habits of mind. Bryan's political base shrank to the former Confederacy as he found allies in conservative Protestant sects and the Ku Klux Klan. Although Kazin ably recounts these developments, he refrains from exploring their broader implications about Bryan's legacy, which, perhaps, had more to do with the toxic mix that sustained the southern wing of the New Deal coalition than with the ideas usually associated with Franklin D. Roosevelt and his "brain trust." But this book is a study of political style, not the fine points of party or ideology. That explains, for example, why Kazin's Bryan appears as both a Democrat and as "a die hard Populist" (p. 148). This may prove disconcerting to readers unacquainted with Kazin's *Populist Persuasion: An American History* (1995), which defines populism as "a mutable style of political rhetoric" (p. 5). Kazin's Bryan proved the grand master of this rhetorical style that appealed to the common person against putative elites, and that was shared across the political spectrum.

Kazin paints the religious world of reform with similarly broad strokes. In Kazin's rendering, Bryan's Protestant beliefs defined the moral dimensions of nineteenth-century reform movements, which were "all led by men and women whose faith motivated their activism" (pp. 303–304). Such claims may baffle readers familiar with the liberal, unorthodox, and skeptical views about religion held by other heroes of reform. Christian socialists such as Edward Bellamy sought guidance from scientific rationalism. Theosophists such as Jacob Coxey channeled Jesus, Vishnu, and Buddha. Secular free thinkers such as Eugene V. Debs, Clarence Darrow, Elizabeth Cady Stanton, Carrie Chapman Catt, and Robert M. La Follette spoke to no god. The same went for the suspected agnostic Charles Bryan, governor of Nebraska, vice-presidential candidate, and political manager for his more famous brother. That Charles wanted no part of the campaign against Charles Darwin suggests the possibility that it was William who was the apostate, having scorned the earlier reform tradition of theological liberalism and heterodoxy.

This is an elegantly crafted, crisply paced, caringly empathetic, and sturdily researched biography. Anyone interested in the Great Commoner and his age of passionate oratory will find it rewarding. As for Bryan's legacy, if readers are left unconvinced by Kazin's verdict, they will appreciate that he provides abundant stimulus for further exploration.

Charles Postel
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John Pettegrew. *Brutes in Suits: Male Sensibility in America, 1890–1920.* (Gender Relations in the American Experience.) Baltimore: Johns Hopkins University Press. 2007. Pp. xi, 409. $60.00.

"Cavemen," the network sitcom, may have suffered extinction, but as John Pettegrew's book well documents, the embrace of masculine primitivism has thrived since the turn of the twentieth century. Pettegrew looks at a particularly rich moment in the United States when the appeal of Darwinian science "naturalized gender differences in the distant past" and "transformed late nineteenth century understanding of man's beastlike nature from metaphor to scientific reality" (p. 16). Rather than attribute male aggression and violence to natural instinct, he vigorously takes to task the biological determinism described by recent evolutionary psychologists such as David M. Buss and Steven Pinker. "Habits of mind" help script what Pettegrew calls normative "de-evolutionary masculinity": the idea that brutishness and violence are masculinity's "fundamental" features. His book is an abundant description of de-evolutionary masculinity—from sport hunting and college football to wartime combat—and its paths to "toxicity"; it follows de-evolutionary masculinity's spread as if it was a "pathology, contagion, or disease" (p. 2).

Beginning with Frederick Jackson Turner and his infamous "Frontier Thesis," Pettegrew examines not only how American male character was tied to space and the natural landscape, but also how masculine virility thought lost by overcivilization was revived by privileging "rugged and primitive forms" of masculine selfhood (p. 40). Pettegrew's reading of Turner, rugged individualism, and Theodore Roosevelt's "crystallization of the warrior ideal" (p. 218) may seem familiar to readers of the period; however, Pettegrew's helpful essay on sources clarifies the book's indebtedness to and difference from the historiography he traces. If Roosevelt embodies the de-evolutionary spirit in its most robust form, from his celebration of the hunter's thrill of the kill to his cultivation of a gang of roughriders, Pettegrew shows readers that even those men who rejected the call of the wild and preferred urban landscapes (most specifically, Greenwich Village) echoed rugged individualism in their embrace of individuality and in their resistance to middle-class conformity. Pettegrew also argues that "unlike feminists, male-authored cultural criticism included a regressive element, a romantic primitivist impulse that in its most exaggerated (i.e., Nietzschean) form sought to erase the weight of consciousness itself" (p. 54). Interestingly, the women who

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make an occasional appearance in Pettegrew's book do not seem destined to mimic these same habits of mind. Vera McGinnis's career as a cowgirl narrates her "struggle against domesticity" (p. 66), while early twentieth-century feminists used the "ideal of autonomy and self-differentiation" as "one of many avenues toward nongendered humanness" (p. 76). Biology may not be destiny, but women in Pettegrew's book seem enhanced by the very habits of mind that savage men.

A chapter titled "Brute Fictions" examines naturalist literature as a microcosm for the display of de-evolutionary masculinity. With readings of Jack London, Owen Wister, Stephen Crane, and Frank Norris, Pettegrew argues that their popularity "went far in advancing killing into the mainstream masculine imagination" (p. 82). Yet, Pettegrew also demonstrates the way in which this literature can complicate easy habits of mind, especially in his readings of Crane and Norris. Unfortunately, the story that Pettegrew tells of modern American middle-class male readership ignores the longstanding appeal of sensation novels to female readers; these thrillers featured crimes, murders, and a host of de-evolutionary drives in regular serialized installments. Danger thrilled men and women alike.

With a capacious chapter on college football followed by one titled "War in the Head," Pettegrew tracks the "passionate celebration of violence" (p. 219) in sport and war. Emerging in the post–Civil War years, Pettegrew reads college football and the fascinating example of Stanford University's team for the ways in which it strengthened the "turn-of-the-century martial spirit" (p. 179). Pettegrew also describes how wartime writings and remembrances from the Civil War, Spanish American War, and World War I cultivated the allure of battle and the "close identification of real manhood with war making" (p. 209).

The book ends with a de-evolutionary tale of sex and violence. Pettegrew links the racist and pathological rage that became turn-of-the-century "lynch law" to the legal recognition of the "uncontrollable heat of passion" defense, which allowed men to protect their marital property rights and excused murder. The legal fiction that Pettegrew describes, in which male passion is privileged in the law, also seems an occasion to challenge a particularly trenchant habit of mind that attributes irrationality and excessive emotion most often to women. I close with a word about the title: a reader might expect from the invocation of sensibility in the book's subtitle to read brutishness in light of some of the rich literature on male sensibility, sentiment, and sympathy that has flourished over the past decade. Nonetheless, Pettegrew's book remains rigorous and passionate in its narration of the historic appeal as well as the immediate dangers of de-evolutionary masculinity.

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David Fort Godshalk. *Veiled Visions: The 1906 Atlanta Race Riot and the Reshaping of American Race Relations*. Chapel Hill: University of North Carolina Press. 2005. Pp. xvi, 365. Cloth $59.95, paper $22.50.

Atlanta has long been marketed by its civic elite as a city with moderate race relations, "The City Too Busy to Hate." In the fall of 1967, however, Dr. Martin Luther King, Jr., began planning a campaign of organized civil disobedience to challenge what he called his hometown's "false image" of racial progressivism. Reverend King sought to dramatize the fact that Atlanta was "behind almost every major southern city in its progress towards desegregation" (p. 276). An assassin's bullet cut King's planned crusade short, and Atlanta moved into the post–civil rights era with race and class inequalities that translated into the fifth-highest poverty rate among America's major cities.

David Fort Godshalk's pioneering study of the 1906 Atlanta Race Riot is a brilliant exposition of the creation of Atlanta's myth of racial progressivism. Godshalk patiently reconstructs the events leading up to the conflagration. However, the strength of this book is in its exposé of what happened in Atlanta *after* the riot. Godshalk demonstrates that historical memory shapes political behavior. In fact, this may be the best monograph ever written on the aftermath of a major race riot in a U. S. city. It deserves the broadest possible audience, and it will critically inform debates in the histories of segregation, urban public policy, and poverty studies.

Godshalk's book joins works such as William H. Chafe's *Civilities and Civil Rights: Greensboro, North Carolina, and the Black Struggle for Freedom* (1980) and George Wright's *Racial Violence in Kentucky, 1865–1940: Lynchings, Mob Rule, and "Legal Lynchings"* (1990) in peeling back layers of obfuscation to reveal how politicians, newspapermen, and business leaders opportunistically used segregation and the threat of racial violence to seize control of public and civic life in the early twentieth century.

Godshalk employs an interdisciplinary framework of social psychology, social history, and political theory to explain the origins of the 1906 riot. He argues that an ethos of chivalry made white men especially prone to paranoia regarding black male assaults on white women. Black on white sexual violence was often fabricated by Atlanta's major newspapers and seized on as a legitimate reason to initiate a massacre of African Americans. Adherence to a code of chivalry that sanctioned the right of all white men to engage in extralegal violence while allowing them to extend a protective umbrella over all white women was about the only thing that white men rich or poor could claim to share in common in Atlanta. This code also allowed white men to reassert control over wives and daughters who were increasingly gaining a level of independence through wage work carried on outside of the home. The chivalric code, Godshalk asserts, "elevated all white men to privileged positions of guardianship over white women and dominance over African Americans" (p. 175)

Godshalk meticulously connects white paranoia concerning black male sexuality with the duel for political

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and labor control in a rapidly industrializing New South city. "In white men's minds," Godshalk argues, "a black man's assault on a white woman also constituted a power grab for black political and social dominance" (p. 40). In fact, white leaders in Georgia used the 1906 riot to argue that peace between the races could only be achieved through the complete removal of black Georgians from political influence. Echoing sociologist Oliver Cox, Godshalk argues that lynching was also used to intimidate black Georgians to accept inferior working conditions and wages. A refreshing aspect of this book is the author's refusal to trap himself in debates over the relative weight of psychological and material factors in the origins of racial violence. In Godshalk's scholarly framework, psychology, politics, and industrialization are all factors in the increasing deterioration of race relations in the post-Reconstruction South.

The Atlanta Race Riot pushed city resident W. E. B. Du Bois and other African Americans more decisively into the national political arena and paved the way for the creation of the National Association for the Advancement of Colored People (NAACP). What is less well known, however, is that Atlanta's white leadership used the memory of the 1906 riot to enforce a profoundly unequal racial peace on the city. Prominent businessmen and public officials used the specter of white violence to accelerate segregation in all areas of the city's life. Atlanta's white moderates convinced themselves that separation was the only way to avoid further racial unrest. Civic leaders leaned on courts to speed up trials involving African Americans accused of crimes, thus undermining the concepts of equal justice and due process.

Jim Crow was papered over by a veneer of racial cooperation between moderate white and black elites in Atlanta who came together ostensibly to avoid further outbreaks of racial violence after 1906. Godshalk is at his best in unveiling the social costs of elite interracial politics in Atlanta. This was a politics largely devoid of substance and it increasingly isolated African American leaders from a mass base in working-class black communities. Godshalk argues that "Interracial cooperation thus stymied the forging of the sorts of intraracial alliances that might have strengthened the position of the entire black population in relation to whites" (p. 161).

Godshalk's book demonstrates that white civic leaders created the official meaning and memory of the 1906 Atlanta Race Riot; it also suggests a new reading of King's "Letter from A Birmingham Jail," which argued that perhaps "the Negro's greatest stumbling block in his stride toward freedom is not the White Citizen's Counciler or the Ku Klux Klanner, but the white moderate, who is more devoted to 'order' than to justice" (p. 276). Thanks to Godshalk we now know much more about the social origins of racial moderation in America.

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Anne L. Bower, editor. *African American Foodways: Explorations of History and Culture*. (The Food Series, number 7.) Urbana and Chicago: University of Illinois Press. 2007. Pp. 183. $35.00.

It has become commonplace to note that culinary history is achieving due academic recognition. The emergence of peer-reviewed journals and food-related book series speak to the field's ongoing incorporation into mainstream historical scholarship. Central to the process of attaining scholarly legitimacy has been the steady effort to place the "culinary" in the context of the "history": that is, to interweave food into the past in ways that avoid the insularity that characterizes so many amateurish exemplars of the genre. These seven essays, edited by Anne L. Bower, offer a microcosm of the field's progression. In their wide-ranging quality and subject matter, they reveal evidence of culinary history's potential as a method of historical inquiry while, perhaps more notably, underscoring the serious flaws to which the discipline remains most vulnerable.

The study of African American history has made heroic strides in the last twenty-five years and thus it makes perfect sense, as Bower correctly frames the matter, to study "the powerful ways food and food customs have shaped group and individual identities" (p. 8). Unfortunately, the first three essays in the volume—included under the rubric "The History of African American Food"—collectively fail to integrate African American food into the detailed tapestry that historians have woven over the past two decades. What follows in part one is a series of compelling culinary observations without the historical grounding to make them much more than anecdotal or, worse, celebratory expressions of African American food. The historical processes underlying identity and group formation are, in essence, absent.

This disconnect takes a unique form in each essay. In "Food Crops, Medicinal Plants, and the Atlantic Slave Trade," Robert L. Hall synthesizes a considerable corpus of secondary literature on African food to provide a useful typography of crop origins. However, his conclusion that "most of the food products discussed here were inextricably linked to slavery" (p. 35) will confound critical readers because the essay conspicuously fails to situate African food in the well-documented framework of American slave labor. William C. Whit, in "Soul Food as Cultural Creation," begins with the claim that food is "constitutive of, and an exemplary performance of, culture" (p. 45) but proceeds to make sweeping historical remarks—i.e., "most slaves cultivated private gardens" (p. 47)—without showing any sort of cultural performance at work or, more problematically, any evidence that the claim is even true (which it almost certainly is not). Finally, in "Excavating the South's African American Food History," Anne Yentsch dutifully promotes the great potential of historical archeology to illuminate African American food customs. However, she also makes claims that quickly reveal her lack of familiarity (despite 120 notes) with

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the defining historical research into African American culture. For example, her assertion that "[m]asters allocated food resources using a value system that cared not one whit for the . . . time constraints . . . of the black community" (p. 61) flies in the face of everything we have come to learn about the task system of labor arrangement throughout the lower South.

Part two, "Representations of African American Food," offers a more convincing, if still disjointed, historical analysis. Arguing that "students of African American literature and culture should pay as much attention to food as to music" (p. 120), Doris Witt traces a post–Civil War timeline of African American culinary literature that parallels the Norton Anthology's canonical organization. Highlighting the busiest intersections of literature and history, Witt's article is not only brimming with astute insights but will be valuable for graduate students searching for viable thesis topics. Especially interesting is Witt's analysis of how cookbooks penned by emancipated blacks strove to undermine the stereotype that "they 'cooked by instinct' and knew not what they did" (p. 107). Psyche Williams-Forson's interest in "cultural sites where food and memory intersect" (p. 135) informs her provocative sketch of the ways in which chicken, "a food only marginally part of [African American] cuisine," became a symbol both of black degradation and resistance. Finally, Rafia Zafar analyzes a handful of domestic manuals penned by "black hospitality entrepreneurs" in order to gain a deeper understanding of an emerging black middle class between emancipation and World War I. While her evidence is frustratingly thin, Zafar does make a number of intriguing suggestions worthy of further investigation. Her claim that African American entrepreneurs secured "independent livelihoods" by tactfully negotiating "the prevailing hierarchical pattern of white master–black servant" (p. 141) dichotomy, for example, warrants a nuanced study.

Bower faced a daunting task in finding a common thread to pull together these diverse articles. It comes as no surprise, then, that her introduction focuses primarily on women, and specifically on "women of the past as historians as well as cooks and food as a major component of culture" (p. 6). Curiously (or not), this theme is scantily (and never systematically) discussed in the six essays mentioned above. Where it is discussed, however, is in the volume's last essay, written by Bower herself, on five cookbooks published by the National Council of Negro Women. Bower's discussion of the ways in which these books enabled black women to be "guardians of history and culture" is exceptionally creative, but—and this evaluation holds true for the volume as a whole—how it relates to the larger scholarly work in African American history remains an unresolved question.

James E. McWilliams
Texas State University

Julian B. Carter. *The Heart of Whiteness: Normal Sexuality and Race in America, 1880–1940*. Durham, N.C.: Duke University Press. 2007. Pp. ix, 219. Cloth $74.95, paper $21.95.

Focusing on marriage counseling literature and public school sex education texts, this book offers a provocative reinterpretation of key elements of the twentieth century sexual conversation. According to Julian B. Carter, sex advisers were the heritors of the previous discourse over neurasthenia, and, beginning early in the century, they both built on and turned around its focus on the ills wrought to the nervous body by civilization. In focusing on marital love and the importance of sexual satisfaction, ideally through the simultaneous orgasm, these counselors imposed both heterosexual norms and white middle-class notions on readers. Sex educators working in the schools in first half of the twentieth century taught young people the dangers of venereal disease and the biology and responsibilities of parenthood, all the while avoiding the subject of desire and imposing the values of a culturally specific heterosexuality derived from the WASP bourgeois world. Just as David R. Roediger found for the labor movement and Louise M. Newman for women's suffrage, Carter finds whiteness to be at the heart of her subject.

But a historian making this particular case comes up against a problem. In neither the marriage nor the sex education books is whiteness explicitly mentioned. Thus the central question is: how does one interpret silence, particularly the silence surrounding race? The author's answer is that silence shouts, and she sets about demonstrating this by tracing a lineage that begins with neurasthenia in the late nineteenth century. Race, Carter argues, was an explicit concern of writers dealing with neurasthenia. Backed by the words of George Miller Beard in *American Nervousness: Its Causes and Consequences* (1881), she establishes this thesis to some degree.

Beard stated that his patients and readers were suffering breakdowns and a host of physical and mental symptoms in the wake of technological progress and economic change. He argued that neurasthenia was a lack of nerve force, and his principal metaphor was economic: ill Americans overspent their nerve capital. Doctors were in business, and Beard's book was likely an effort to expand his patient base. Thus he reassured readers and potential patients that their troubles did not portend insanity and that they happened only to those in the higher reaches of the social order: the working class and African Americans were exempt. Beard was clearly class and race biased, but race was largely a way to highlight the gentility of his intended clientele. Although his book became a big seller, Beard was controversial among nerve specialists in his time, who regarded him as less a scientist than as a promoter of electrical treatments. His neurologist colleagues were also prejudiced and typically drew on the evolutionary science of the time with its deep racism. But their concern was the diagnosis and treatment of nerve illness: their racism was a subordinate part of their argumentative framework, not its impelling force.

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This is important to emphasize because Carter's book depends on the structure and weight of explicit racism in nineteenth-century neurological discourse to make the case for the significance of the silence over race in the sexual conversation of the twentieth. As new voices emphasized the positive power of sex imagined within marriage, writers did not identify the race of their audience. To Carter, this created a "race-evasive" discourse in which instructors attempting to assist couples attain sexual fulfillment were key players in the project of white racial dominance. But could their writings be linked more appropriately to new appreciations of female sexuality, revitalized understandings of human sexual needs, and, in some cases, an ideology that put sex at the center of life? Might the emphasis on heterosexual marriage bear a relation to desires to evade censorship codes? What about the reshaping forces of the 1920s and 1930s that included, for example, changes in female dress, a growing enthusiasm for jazz and the movies, and the impact of Sigmund Freud?

The chapter on sexual education texts in the schools has excellent elements, including the discussion (needed at the outset of the book) about the diverse sexual cultures of the United States. Carter creates the wonderful term "frank reticence" to describe the way the texts managed to avoid eroticism as they gave information about sexuality. After eschewing the initial focus on venereal disease, a potential scare tactic, texts turned to the birds and bees, approaching human sexuality from standpoint of evolutionary biology. Carter offers interesting examples showing the way members of the animal kingdom were used to illustrate the values of middle-class family life. As sex education moved within schools, it, too, was silent about race. Does that mean that when family is mentioned it needs to be read as "white family"? Not only does Carter think so, but here and elsewhere she restates arguments to interject whiteness or white (in one of its forms) as a modifier. For example, following a quote on love of mate and children, she writes: "the sexual urge was the ancient raw material out of which generations of white couples had gradually crafted love, civilized self-restraint, and modern family life" (p. 148). Would the middle-class African American women described by Evelyn Higginbotham and Glenda Elizabeth Gilmore have accepted this racial modifier? That is not to say that there were not strong racial elements involved in the twentieth-century sexual conversation. Discourses of race suicide and eugenics engaged race and class powerfully, explicitly, and painfully, as did justifications for lynching and discussion about the laws against miscegenation. These, however, are not the subject of Carter's book.

To a lesser degree, this study focuses on heterosexual norms, and here Carter's discussion is on solid ground. The married couples being counseled were heterosexual, as were the birds and the bees in sex education textbooks. The icons pictured on the book's cover—Norman and Norma, terra cotta nude statues of the average male and female displayed at the 1939 World's Fair and discussed in the introduction—are never considered to have any other than heterosexual leanings.

In sum, this is a suggestive book with much valuable information, but, to this reader, it fails to make its particular case for the sounds of silence.

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Smith College

Pippa Holloway. *Sexuality, Politics, and Social Control in Virginia, 1920–1945*. Chapel Hill: University of North Carolina Press. 2006. Pp. xi, 258. Cloth $59.95, paper $19.95.

One can understand how the fashionability of sexuality studies and the race-gender-class approach to explaining the American past led Pippa Holloway to Virginia, the state that took the lead in seeking to sterilize patients in mental institutions. When the United States Supreme Court put its stamp of approval on the now notorious law in *Buck v. Bell* (1927), thirteen states enacted similar legislation.

Holloway places this law in the context of other legislative action during 1922–1924, seeking to demonstrate how elite whites sought to control the masses by emphasizing the sexual danger they posed. To the Virginia sterilization law she adds the Racial Integrity Act, which widened the definition of who was black to include anyone with a black ancestor, no matter how distant, for the purpose of preventing interracial marriage, and a law establishing a movie censorship board charged with denying a license to films judged to be obscene or indecent. She then covers the landscape through World War II, spending most of the book on the subject of venereal disease. And here, since Holloway sees the campaign for its treatment in terms of paternalism and insuring a docile workforce, she questions the motives behind the provision of free clinics ministering to the needs of the infected poor.

Holloway recognizes that sexual regulation was justified in terms of promoting economic growth and protecting the public health, but she seems to believe that such reasons were largely window dressing for attempts to control the disenfranchised. She asserts that "white elites seized any opportunity to reinforce their dominance" (p. 111), including the targeting of certain groups for venereal disease testing.

Although the 1922–1924 laws all tended to receive similar support, she acknowledges that their effect was limited by practice. In fact, throughout the book she regularly contends with anomalies that instead of advancing her thesis question its viability. At times she takes refuge in the view that those who construct race, class, and sexuality by that very construction deny to others their full rights of citizenship. That such rights were denied to both blacks and whites in Virginia in the period is a fact, but Holloway has not made the case that sexual regulation played an important part in this story. She suggests that the ruling elite was hesitant to finance new programs and was wary of increasing governmental interference in people's lives. Furthermore, four of her

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seven chapters deal in various ways with venereal diseases, where regulation indeed may first have been placed on those without political power but where concerns about public health are real and immediate. In addition, she notes that when legislation affected the middle class, objection arose to either derail or modify the attempted regulation. Attempts to prevent the communication of birth control information, to provide for the registration of all citizens on the basis of race, and to regulate barber shops all failed. And when the opposition could not defeat a proposal, such as premarital blood tests for syphilis, the legislation was substantially changed.

We can all agree that governing elites seek to retain power and protect their own interests, generally with little concern for those without access to political power. This hardly was a distinctive characteristic of the elite in Virginia during this period of time. Where the historian can make a contribution is not by asserting the obvious but by detailing how such control was effectively exercised, and how it contributed to maintaining the elites in power. Here the author has confessed that the execution of such laws did not fulfill the hopes of their framers, and the change she traces comes not through the repeal of such laws but through exterior developments tied to the New Deal and World War II.

There are some annoying errors in the book, two of which are in the epilogue. Holloway errs in saying that federal full-employment legislation was not passed, ignoring the Employment Act of 1946. And without any justification whatsoever, she pronounces an attempt on the part of the federal government to limit servicemen's access to prostitutes unconstitutional.

By trying to force disparate strains into some coherent whole, Holloway has distracted the reader from the contributions she makes. What she has produced is a generally well-researched look at certain aspects of the attempted regulation of sexual life in Virginia from 1922 through World War II. She deals with federal-state conflict, the American attraction to eugenics, the resistance to expanding governmental services, the neglect of the powerless, the schizophrenia engendered by the issue of birth control, and, finally, the changes brought by the political and social environments of the war years. This case study draws out matters of concern that do not stop at the state's borders, and it does contribute to our understanding of the decades treated.

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STEVE J. WURTZLER. *Electric Sounds: Technological Change and the Rise of Corporate Mass Media*. (Film and Culture.) New York: Columbia University Press. 2007. Pp. xi, 393. $34.50.

All of us are familiar with the ways in which the digital revolution is reshaping our current media environment, be it through the uses of music, innovations in the film industry, or its impact on radio and television broadcasting. Steve J. Wurtzler reminds us that media revolutions like these are not new and have fundamentally restructured our media uses and experiences in previous eras. The transformation that he focuses on took place in the early decades of the twentieth century, when the introduction of electric sound, first through the telephone and the phonograph, followed by the emerging radio industry and Hollywood's conversion to sound movies, forever altered Americans' media experiences and practices.

The strength of Wurtzler's book lies in its interdisciplinary nature and in the fact that it combines fields generally treated separately, such as the history of the phonograph, radio broadcasting, and the film industry. Wurtzler calls his approach an example of the "technological history of media" (p. 14), which highlights the cross-disciplinary nature of explorations that combine history of science with cultural and media studies. Throughout his discussion he emphasizes that although significant and even dominant at times, large media corporations did not by themselves decide upon the uses of new technologies: "Upon innovation, media technologies have multiple, often conflicting identities. The ultimate meanings they take within social relations are the product of contestations and struggles" (p. 15).

Chapters one and two trace the technological innovations of electrical sound technology as well as the dissemination and publicity surrounding these new media practices. Wurtzler recalls the many debates inspired by these innovations, especially the hope and call for a "new acoustic consumer democracy" (p. 2). And while some options became reality and coexisted next to each other, Wurtzler also makes it clear that corporate giants like RCA, AT&T, and General Electric used their market dominance to exert control over these new industries, both through strategic acquisitions and selective prosecution of patent rights. Advocates for an expanded, participatory media democracy frequently had to be satisfied with niche markets and a marginal existence and were easily trounced by the dominant commercial media. Media conglomerates, tie-ins, and synergy were already well established patterns by the early 1930s. RCA, for example, owned the film studio RKO and the Victor phonograph company as well as controlled the NBC radio network, and it used these assets for concentrated and coordinated media campaigns.

In chapter three Wurtzler examines the links between and impact of already established media practices on newly emerging ones. Several themes of this insightful analysis are particularly noteworthy. One relates to the relative quick adoption of phonograph usages by the emerging radio industry in the 1920s. The fact that the radio set quickly emerged both as a musical/entertainment instrument as well as a priced piece of furniture was modeled after the phonograph industry, which already had made the same transition by the late 1910s. In the process, the technological workings of the new instruments were gradually disguised, accelerating what the author calls "the dissimulation of the machine" (pp. 120–121). Another intriguing example of

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cross-media adoption was the brief but failed experiment with sponsored films in the early 1930s, discussed in the ensuing chapter. Inspired by the success of radio sponsorship, film studios explored commercial sponsorship of films. Several national manufacturers and advertisers signed agreements with Paramount and Warner Brothers and produced films extolling the power of the disinfectant Lysol (*A Jolt for General Germ*) or the adventures of tea and coffee cultivation (*On the Slopes of the Andes*). Ultimately, both advertisers and studios agreed that product placements in films were by far more effective and less intrusive forms of sponsorship, discontinuing commercially sponsored films.

The discussion in chapter four picks up on the explorations of chapter two and analyzes the conflicts and struggles that decided the social roles the new media would perform. Exploring aspects such as the work of the Committee on Public Information (CPI) during World War I, the development of a network-dominated radio industry, and the establishment of the Federal Radio Commission in the late 1920s, Wurtzler confirms what a number of media historians have argued before him: "Acoustic media could promote democracy—but democracy understood as 'consumer democracy' in the first instance and only secondarily cultural and political democracy" (p. 224).

In the final chapter, the author traces the sound battles that were being fought in a number of these industries during the 1920s especially. At the heart of the controversy was the question whether the first responsibility of electric acoustics was to accurately record and replay sounds (transcription paradigm) or whether media could use sound for novel expressions and even illusion (signification paradigm). Wurtzler convincingly demonstrates that the initial preponderance of the transcription model, where audiences wanted primarily authentic and untainted recordings, slowly gave way to what the author refers to as "signifying fidelity" (p. 268). This meant, for example, that Hollywood recording engineers increasingly expanded their "arsenal of tools through which they might technologically mediate sound so as to signify fidelity to an original event" (p. 276).

Even though a number of Wurtzler's arguments retrace those advanced by other media historians, this book expands our understanding of a critical era of media history and is particularly valuable in terms of understanding cross-media influences. In his conclusion, Wurtzler also analyzes the parallels between the electric sound revolution and the digital revolution that we are are currently undergoing and highlights what we can learn from past experiences. All of these aspects make the book an important contribution to the history of technology as well as media studies.

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SARAH E. IGO. *The Averaged American: Surveys, Citizens, and the Making of a Mass Public*. Cambridge, Mass.: Harvard University Press. 2007. Pp. 398. $35.00.

The 2001 Princeton dissertation from which this book came won an important award, the book won its own award as the first by a young scholar, and it has been widely reviewed. That Sarah E. Igo is a talented younger historian is no secret, and she gladly shares the limelight. Her heartfelt acknowledgements fill six full pages, wherein some of her benefactors are named twice. One could imagine, were one writing an intellectual history of our time a century hence, that she might serve as a cynosure of this academic era, especially at the higher reaches of the trade: female, fair-minded, scrupulous and thorough, careful to sidestep ideologically loaded positions, respectful but not adoring of the past, skilled prose stylist, gently humorous. And most intriguing perhaps, she is a historian of ideas whose data are works of modern social science no longer read nor much remembered by today's practitioners of same. She is their Boswell to a forgotten Johnson.

Igo's book is a triple case study, wherein she examines the initial public reception and subsequent uses made of Robert and Helen Lynd's two Muncie books, early Gallup and Roper popular opinion polling, and Alfred Kinsey's sex studies. *Middletown* (1929) and *Middletown in Transition* (1937) were Book-of-the-Month bestsellers and staples of reading groups in their time, and that as such, so Igo argues, transformed the public's understanding of social science in a very positive direction.

To be sure, the books were reissued in the 1950s and are famous by name, but they are no longer studied nor valued as classics in the way, say, that *The Polish Peasant in Europe and America* (1919) by William Thomas and Florian Znaniecki is so regarded. Despite some grumpy resentment expressed by residents of Muncie, Indiana, where the Lynds and their staff camped out for a couple of years, the rest of the country found the books irresistible because of their "key-hole" examination of small-town life regarding jobs, families, religion, education, recreation, and community functions. And despite the fact that neither of the Lynds had studied sociological field methods, their academic futures were secured on the basis of these "amateur" productions. Whereas Sinclair Lewis's *Main Street* (1920) and *Babbitt* (1922) had trod similar ground, his works made no attempt at "balanced objectivity," while the Lynds' entire *raison d'être* turned around their repeated claim that *Middletown* described what was indeed the case, and not what someone wished it to be. It was an anthropological portrait and not a normative paean to this or that position.

It is clear that Igo enjoys retelling the story of the Lynds' ethnographic adventure, she prizes what they accomplished, and she believes the books have continuing importance. They are pivotal for the history of modern social science as well as revealing the roots of to-

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day's broader culture. Her term "average" has caused other reviewers to wonder if "mean, median and mode" are inaccurately conflated with "typical," but I sense they miss her point, which she expresses well. She finds it interesting that until the Lynds, the pollsters, and Kinsey published their works, American popular society did not conceive of itself as representable by a typical *Weltanschauung*. In fact, it bridled at this suggestion since the American ethic as identified by Alexis de Tocqueville and others principally revolved around independence of mind and body. To be told in "scientific" terms that in fact a great deal of conformity to group norms occurred in Muncie—which Robert Lynd's Veblenian critique of consumerist culture found objectionable, partly because Lynd himself grew up in a similarly constrained environment—confronted middle America with the indisputable fact of its own submission to the inroads of industrialized consumption.

What Gallup, Roper, and the J. Walter Thompson advertising agency similarly discovered was that products could be tailor-made to fit purchasers' demands, and, better yet, the same purchasers could be persuaded to buy what they did not need. When the Lynds chose 1890 as their point of comparison with the "roaring '20s," they knew in advance that the social environment of their parents and grandparents still partook of what sociologists call *Gemeinschaft* or village interaction, but following World War I, this social fabric had been shredded, not least by the car, radio, movies, and advertising. Rereading *Middletown* today is a stunning revelation since, for example, so many of the arguments provoked by Muncie's adolescents' desire for mobile freedom began in the 1920s and have remained in play ever since, virtually word for word. Perhaps it was reading these works that inspired Igo to write about them with such careful attention to detail, and to portray them in a sympathetic voice.

Igo's treatment of the pollsters and of Kinsey are equally well done, though perhaps a tad less interesting since we continue to live with daily polling and with daily sexual portraits of our society, whereas *Middletown* is of a distinctly foreign locale. On a recent blog, Igo reports about her own revelation when visiting Muncie and Bloomington, Indiana, as a graduate student, shocked at being treated with astonishing kindness and helpfulness. The Midwest is still another country, and it is good of Igo to remind us of that.

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Judy Kutulas. *The American Civil Liberties Union and the Making of Modern Liberalism, 1930–1960*. Chapel Hill: University of North Carolina Press. 2006. Pp. xii, 305. $35.00.

Histories of the American Civil Liberties Union (ACLU) provide more fodder for the common observation that the ugliest building at most universities invariably houses the Architecture Department. The organization best known for championing commitments to democratic freedoms in the United States has had a long history of undemocratic internal organization. Roger Baldwin, the long time president of the ACLU, treated the group as a personal fiefdom, following his whims regardless of whether they were sanctioned by the Board of Directors or the membership. Prominent directors believed Americans ought to be governed democratically, but not ACLU affiliates. "The more liberal ACLU leaders became," Judy Kutulas's study of ACLU politics during the Depression, World War II, and the McCarthy era details, "the less they trusted the membership" (p. 11). In her view, "[t]he idea of organizational democracy was a hard concept for many [ACLU] officers, whose mission was protecting dissenters against the tyranny of the majority" (p. 190).

Kutulas meticulously details the ways in which the leadership of the ACLU struggled with internal and external issues associated with democracy during the mid-twentieth century, the time when that organization "journey[ed] from the radical fringes to the liberal mainstream" (p. 2). Some controversies were over the role of both communists and affiliates within the ACLU. Other debates centered on whether organizational leaders should seek only to refine national policy made by liberal elites in the Roosevelt or Truman administrations or to continue the direct protest style that characterized the ACLU's early years. None of these struggles do great credit to American civil libertarians. During the early 1940s, the anticommunist leadership of the ACLU purged the board of persons who belonged to the Communist Party and passed a resolution banning communists from future board membership. Several prominent national board members helped the House Un-American Committee spy on affiliate members who were thought too red for civil liberties work in the United States. The national board supported or avoided taking stands on such issues as the internment of Japanese Americans, President Harry Truman's loyalty programs, and the execution of the Rosenbergs. One board member during World War II, defending the ACLU's restrained stance, claimed that "[t]he Union can be of greater practical service in days ahead . . . if it makes certain concessions to the general consensus of opinion in regard to the 'National Emergency' " (p. 98). What civil liberties activism took place in these years was largely initiated by more radical, local ACLU affiliates, most notably the Southern California Civil Liberties Union. Not only did the national board often decry such efforts, but the Chicago Civil Liberties Bureau was decertified when evidence emerged that communists had too much influence on daily activities.

Kutulas believes that the ACLU's moderation was largely a consequence of second-generation leadership, which was more comfortable in the halls of power than the organization's radical founders. The founders had believed government was the natural enemy of freedom and regarded themselves as the champions of the oppressed. The Great Depression and the Roosevelt administration dramatically increased the stature of the ACLU, but with increased stature came increased co-

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optation. Board members were comfortable liberals, whose idea of protest was filing an amicus brief before the Supreme Court. These civil libertarians often seemed more interested in establishing their respectability than in challenging the democratic pretensions of political leaders. The ACLU "offered a safe, responsible, meaningful engagement with issues" (p. 41) that enabled mainstream progressives to burnish civil libertarian credentials without championing dramatic political reforms. Even old radicals become tamed by the prospect of respectability. One would hardly recognize the Norman Thomas of the 1930s, who thought the New Deal too timid, in the Norman Thomas of the late 1940s, who fought to remove any taint of communist association from the ACLU.

The history in this book is far better than the analysis. Kutulas has done a magnificent job unearthing the internal debates between ACLU members over how democratic the organization should be and what democratic freedoms the organization should fight for. Her study is particularly valuable for detailing all the different progressive variations played by ACLU affiliates, both in terms of organization and in terms of what a commitment to civil liberties entailed. When documenting the consequences for the ACLU of professionalizing a radical organization, however, Kutulas does not engage with social scientists who write about the nature of interest groups or even the literature on the organization problems of left-wing interest groups. The book also is not interested in the content of the ideological struggles between mid-twentieth-century liberals. Kutulas presents a good deal of information about the power struggles that took place between various anticommunist factions on the ACLU, but very little on the theoretical differences between them. One consequence of the narrow focus is that readers do not get a true sense of the role of these debates in the making of modern liberalism between 1930 and 1960.

The American Civil Liberties Union has been a subject of scholarly investigation across many disciplines, and Kutulas has made a useful contribution to that dialogue. Not surprisingly, her book engages with other works on the ACLU and makes important contributions to understanding the tensions experienced by an organization that seeks mainstream credentials while protecting political dissent. After blaming Kutulas for not writing the book on the ACLU they would have written, persons engaged in different disciplinary dialogues on civil liberties in the United States will find their conversations enriched by reading this one.

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Charles Thorpe. *Oppenheimer: The Tragic Intellect.* Chicago: University of Chicago Press. 2006. Pp. xx, 413. $37.50.

J. Robert Oppenheimer (1904–1967), a brilliant physicist (sans Nobel Prize) and polymath, was best known as the director of the Manhattan Project's Los Alamos Laboratory, which invented, designed, and built the atomic bombs dropped on Hiroshima and Nagasaki. In 1954, Oppenheimer was humiliatingly stripped of his security clearance in McCarthyite hearings that psychologically broke him. He reconstituted himself as a public moralist, speaking in an oracular style that many found mesmerizing. Oppenheimer was complex: respected physicist and teacher at the University of California, Berkeley, and Cal Tech; utterly charismatic leader at Los Alamos; attractive to women; respected for his integrity but not always truthful; at once cultured, sophisticated, handsome, awkward, arrogant, emotionally distant, deeply caring, and self-loathing. The fascination with Oppenheimer has spawned a vast historical literature including Kai Bird and Martin J. Sherwin's richly detailed, Pulitzer Prize–winning biography *American Prometheus: The Triumph and Tragedy of J. Robert Oppenheimer* (2005).

In his daring, concise, and sharply etched portrait of Oppenheimer, Charles Thorpe moves beyond the current literature to explore both Oppenheimer's character and his public persona. Thorpe writes what he terms a "sociological biography" (p. xv) that not only places Oppenheimer's life in larger social and political context—a difficult endeavor that is a mark of all good biography—but uses Weberian analysis to examine the more immediate social processes through which individual character and self-presentation are shaped. Thorpe presents the full arc of Oppenheimer's life, including his childhood and education, concentrating on his leadership at Los Alamos, the postwar security hearings, and Oppenheimer's reconstituted self and public persona.

Thorpe pays particular attention to charisma, an adjective often used in describing Oppenheimer, particularly his leadership at Los Alamos. But while others have demonstrated Oppenheimer's charisma, no one writing before Thorpe has analyzed it in the careful Weberian sense not as an inherent personal quality but as a collective and collaborative fashioning: in this case, as a co-construction that took place in the intensely challenging web of political, cultural, and social relationships at Los Alamos during the war. Thorpe shows, for example, how senior scientists at Los Alamos pushed and supported Oppenheimer to become more decisive (early on, one of his closest friends thought "He couldn't run a hamburger stand" [p. 68]). And Thorpe shows how Oppenheimer's charismatic role emerged as he mediated between scientists and the military and as he showed again and again an uncanny ability to understand and explain the whole and each part of the enormously complex atomic bomb project.

Thorpe also analyzes charisma in relation to the security hearings and after. During the hearings, which were fueled by deep personal enmity and the political determination to roll over opponents of the hydrogen bomb, Oppenheimer was imputed to have used char-

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ismatic influence to hinder the H-bomb program. Thorpe shows how, after the public humiliation of having his security clearance stripped, Oppenheimer cultivated a different kind of charisma as a public figure who was seer and conveyer of nature's mysteries. Oppenheimer also let stand the portrayal in popular books of him as having suffered a sense of deep sin at the moment of the first test (Trinity) of the atomic bomb in July 1945. These accounts had Oppenheimer thinking lines from the Bhagavad-Gita, "I am become death, the shatterer of worlds," although Oppenheimer's brother Frank, who was at the Trinity test with him, recalls that they both simply said that it worked.

Thorpe writes at somewhat greater distance from Oppenheimer than do, for example, Bird and Sherwin. Presenting an absorbing narrative, Thorpe often pulls back to ask, and answer, questions about larger social processes, Oppenheimer's character formation, and Oppenheimer's actions—whether micro-interactions with others, his pattern of leadership at Los Alamos, or his presentation of self. With Thorpe, we stand not next to Oppenheimer but facing him. We have far less of a sense—or illusion—of experiencing life through Oppenheimer's eyes than we do with Bird and Sherwin, including less sense of his love affairs and his troubled relationships with his wife and two children. What emerges is a darker and somewhat less sympathetic portrait than in *American Prometheus*. On the one hand, Thorpe presents a more consciously constructed and not entirely attractive public persona. On the other, Thorpe uses observers to suggest some of the workings of Oppenheimer's psyche, including a false self. For example, Oppenheimer's very close colleague I. I. Rabi says that he could find no underlying unity to Oppenheimer and that Oppenheimer "lived a charade" (p. 17).

Thorpe's challenging portrayal is convincing, though a character as complex as Oppenheimer's inevitably invites further probing. Left unanswered here is precisely how Oppenheimer moved from being indecisive at Los Alamos to extraordinarily effective. But this is an invitation to further work, not a shortcoming of an original, highly intelligent, well-researched, well-written, and deeply satisfying book.

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JAMES PECK. *Washington's China: The National Security World, the Cold War, and the Origins of Globalism*. (Culture, Politics, and the Cold War.) Amherst: University of Massachusetts Press. 2006. Pp. 333. Cloth $80.00, paper $24.95.

As a senior editor at Pantheon Books and also as the director for the Culture and Civilization of China publishing project, James Peck has helped to guide the publication of numerous books that explored the evolution of modern China's political culture in ways that often challenged prevailing notions of China as a "rogue" state and implacable American enemy. In his own book, Peck attempts to link the pre-1969 efforts by America's national security managers to isolate, subvert, and topple the People's Republic of China (PRC) to larger trends in United States Cold War policies, especially what the author terms America's "visionary globalism."

Unfortunately, much of what appears seems to be caught in a historical time warp, arguing positions and evidence no longer in serious dispute. Peck's assertions and documentation are not so much invalid as anachronistic. For example, as part of a long introduction, he notes the cruel and destructive impact of Senator Joseph McCarthy's attack on the Foreign Service's "China Hands" and their subsequent marginalization along with journalists and scholars intrepid enough to empathize with aspects of China's revolution. Surely, this is a valid point, but hardly a novel one.

Later chapters chronologically and thematically explore efforts by the Truman, Eisenhower, Kennedy, and Johnson administrations to contain, isolate, and, ultimately, overthrow Mao Zedong's regime. Along the way Peck notes how Washington's demonization of Chinese Communism frustrated allies, alienated possible friends in the developing world, and led the United States down some terrible blind alleys, none more awful than Vietnam. To buttress his thesis, Peck relies extensively on documents released through the Declassified Documents Reference System, mostly during the 1970s and 1980s, as well as a collection of National Intelligence Estimates released by the Central Intelligence Agency after the collapse of the Soviet Union. While important, these are only a small sample of a much larger documentary universe available to scholars beginning in the early 1990s. Reliance upon such a narrow sample tends to oversimplify and distort Peck's thesis. For example, by concentrating on Secretary of State John Foster Dulles's public denunciations of "Godless Communism," the author overlooks the far more nuanced policy debates over China that President Dwight D. Eisenhower encouraged. Similarly, failure to examine the growing body of documentation about China's actual foreign policy leaves the reader clueless as to whether Washington's hard line was completely off base or partially justified.

Missing from this book are references to and reliance upon a broad array of primary and secondary materials that other scholars have parsed to evaluate Sino-American relations from 1949–1969. Peck does not appear to have directly utilized the rich China policy files available at the Truman, Eisenhower, Kennedy, and Johnson presidential libraries, the extensive files on China policy made available by the National Security Archive at George Washington University, the documentation both published and online amassed by the Cold War International History Project, nor even many relevant volumes of the Foreign Relations of the United States series published by the State Department.

Since the early 1990s, at least a dozen U. S., British, and Chinese scholars have published monographs with far broader documentary bases that cover many of these points, often from a multilateral perspective. In no particular order, these include Rosemary Foot's *Practice of*

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Power: U. S. Relations with China since 1949 (1995), Qiang Zhai's *The Dragon, the Lion, and the Eagle: Chinese-British-American Relations, 1949–1958* (1994), Shu Guang Zhang's *Deterrence and Strategic Culture: Chinese-American Confrontations, 1949–1958* (1992), Thomas J. Christensen's *Useful Adversaries: Grand Strategy, Domestic Mobilization, and Sino-American Conflict, 1949–1958* (1996), Robert Accinelli's *Crisis and Commitment: U. S. Policy toward Taiwan, 1950–1955* (1996), Odd Arne Westad's *Brothers in Arms: The Rise and Fall of the Sino-Soviet Alliance, 1945–1963* (1999), Nancy Bernkopf Tucker's *China Confidential: American Diplomats and Sino-American Relations* (2001), and Chen Jian's *China's Road to the Korean War* (1996) and *Mao's China and the Cold War* (2001). The list could go on.

These lapses, unfortunately, diminish the value of Peck's book as either a reliable reference work or an up-to-date teaching tool.

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Donna Alvah. *Unofficial Ambassadors: American Military Families Overseas and the Cold War, 1946–1965*. New York: New York University Press. 2007. Pp. xi, 291. $42.00.

In this study Donna Alvah seeks to understand both the reasons behind the U. S. decision to station military families overseas in the period 1946–1965, and the impact and significance of their presence. In the aftermath of World War II, U. S. soldiers stationed overseas clamored to return home in order to pursue the promise of domesticity, long deferred by wartime service and increasingly central to the American way of life. With military imperatives demanding the continuation of a substantial U. S. presence overseas, the notion of sending the wives, children, and even fiancées of servicemen overseas began to gain currency. Those in favor of facilitating the stationing of military families overseas argued that the presence of family responsibilities and comforts would serve as a panacea against poor morale, increased immorality, and crime within the U. S. military forces. Representative Margaret Chase Smith petitioned Secretary of War Henry Stimson to allow servicemen's wives and fiancées to join them overseas in order to boost morale and efficiency. She warned that, in the absence of American women, American servicemen were liable to fraternize with German women, thus permitting the spread of "Nazi Ideals" (p. 24).

While the stationing of U. S. military dependents abroad may initially have been relatively narrowly conceived as a way of improving troop morale and recruitment and retention rates, Alvah identifies a broader significance to what might, with some justification, be called the "deployment" of so many American wives and children overseas. The stationing of American*families*, rather than just American servicemen, she argues, served an important ideological purpose. Drawing on Joseph Nye's conceptualization of "soft power," Alvah argues that American families abroad served as unofficial ambassadors demonstrating both the benefits and the benevolence of the United States. At a four-hour orientation program, spouses and adolescent children arriving in Germany were warned, "every American man, woman, and child in the European Theatre has the power to do either good or harm to our foreign relations . . . a thoughtless, careless or insolent attitude can make an enemy of a former friend or a future friend" (p. 38). Alvah's account of the directions and encouragement given to the wives and children of American military personnel is well-researched and keenly observed. The *U. S. Lady* magazine, a publication aimed at military wives, carried a series of advertisements between 1956 and 1959, sponsored by the Air Force, which lauded the nuclear family as the "real heart of the U. S. Air Force" (p. 67). As Alvah points out, the pictures in these ads presented the U. S. serviceman as first and foremost a family man. In a pictorial representation of "soft power," the Air Force sought to present itself as "a corps staffed by fathers and husbands, a military body that drew its strength from families, rather than relying on force alone" (p. 70). As consort to this idealized paternalist figure, *U. S. Lady* selected a "Lady-of-the-Month," frequently lauded for her activities off base and efforts as an unofficial ambassadress. Recognizing the continued significance of racial and class issues in the definition of idealized American domesticity, Alvah notes that between 1955 and 1968 only one of these "Ladies of the Month" was African American.

In addition to *U. S. Lady* Alvah draws on a wide range of sources to convey both the experience of U. S. military families overseas and the wider significance of this experience as an expression of U. S. Cold War ideological imperatives. The participation of American servicemen's wives in charity efforts, religious worship, and educational work is examined, as are their interactions with local populations as employees and as friends. Moreover, in contrasting U. S. experience in West Germany and Okinawa, Alvah makes a convincing case for the extent to which historical context and, more powerfully, racial identity affected interactions between Americans and local populations in different settings. Assessing the reactions of occupied populations to these American families, is, as Alvah admits, a harder task, with source material being thin. Alvah makes good use of cultural texts, such as Ōshiro Tatsuhiro's novella, *The Cocktail Party* (1962), which not only exposed the huge imbalance of power between Americans and Okinawans but also criticized the extent to which American attempts at interaction were little more than efforts to disguise this imbalance. "It isn't just the crime of one American that I want to indict," states the protagonist, "But all the pretence of the cocktail party" (p. 195). Alvah's efforts to unpick the metaphorical "cocktail party" and to explain its significance within U. S. Cold War policy are both well researched and insightful. Her study makes a valuable addition to

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the growing literature that seeks to explore the wider meanings of American power.

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ROBERT J. COOK. *Troubled Commemoration: The American Civil War Centennial, 1961–1965.* (Making the Modern South.) Baton Rouge: Louisiana State University Press. 2007. Pp. xiii, 300. $45.00.

As is well known, in order to protect slavery from the political processes of the United States, southern states seceded and established a slaveholding Confederate States of America. The United States, of course, did not acknowledge secession and fought a war with the Confederacy to defeat it, and thereby save the United States as a nation. The war was unusually bitter and bloody, producing in excess of 600,000 military deaths and many grievously wounded military veterans. The manufacture of artificial limbs was a major industry in the South after the war.

In the course of the war, the North defeated the Confederacy, and the slaves were freed. For several years after the war, the United States attempted to protect freed slaves and to integrate them into the southern community as American citizens. This attempt was vigorously and sometimes violently resisted by the southern whites. After efforts at integration failed, the United States government left the freed slaves and southern state legislators to their own devices, which resulted in an apartheid society in the South and a cultural and political separation between the sections of the country. African Americans were severely discriminated against in the South (and the North as well).

In 1961–1965, premised on a Joint Resolution of the Congress, the United States undertook to commemorate the Civil War on the occasion of its centennial. The commemoration was broadly based with the states participating. Robert J. Cook provides a brilliant and detailed account of the unlikely and significantly flawed centennial commemoration.

A national Centennial Commission was appointed. Fortunately, among others, four professional historians were named: Bruce Catton, Allan Nevins, Bell Irvin Wiley, and John A. Krout. Unfortunately, the first chair of the commission and its first executive director were of limited ability. The executive director was a public relations type who saw the commemoration as something that he could "sell" to the public and intended to commercialize it. The commission undertook to mark major events of the war as their anniversaries occurred. These events took place in the midst of a vigorous national civil rights movement, including a spirited attack on racial segregation, the South's principal racial technique. In this atmosphere, the commission often tiptoed carefully to keep the commemoration going as it marked such events as the inauguration of Jefferson Davis as president of the Confederacy, the first battle of Bull Run, the Emancipation Proclamation, and the Gettysburg Address. But in 1964, the commission's luck ran out. A national convention of the centennial observance was scheduled for Charleston, South Carolina, headquartered in a hotel there. An African American member of the New Jersey State Commission was excluded by the hotel in accordance with southern segregation traditions.

The exclusion of the black woman provoked the Kennedy administration's protest and a National Association for the Advancement of Colored People (NAACP) reaction, among other things. The convention was moved to the United States Naval Station at Charleston. The Charleston incident resulted in the removal of the unsatisfactory executive director and the consequential resignation of the chair of the commission. The latter was replaced by Nevins. The place of the executive director was taken by James I. Robertson, a southerner and a graduate student of Wiley. Under their leadership, the commission was able to carry through to the end of the centennial which occurred at Appomattox, Virginia, in 1965.

As indicated by the foregoing account, the word "troubled" is a charitable adjective for the commemoration. Given the sectional and racial differences in the United States, marking the centennial may have been a bad idea. More realistic planning would have anticipated the inevitability of conflicts in the revisiting of the war. Having told the centennial story, Cook concludes with a statement of the films and books that purport to tell the history of the Civil War. These are named, briefly characterized, and evaluated with respect to their historicity. Cook specializes in teaching and research in U. S. civil rights history. He has considerable knowledge of the field and a strong grasp of American racial attitudes and problems.

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THOMAS F. JACKSON. *From Civil Rights to Human Rights: Martin Luther King, Jr., and the Struggle for Economic Justice.* (Politics and Culture in Modern America.) Philadelphia: University of Pennsylvania Press. 2007. Pp. 459. $39.95.

Thomas F. Jackson presents a secular view of an American religious icon. Jackson wants his readers to recall what contemporaries and other movement leaders never forgot: Martin Luther King, Jr.'s radicalism. In so doing, Jackson portrays the civil rights leader as a politically astute activist, a perspective often downplayed or forgotten in other biographies and studies that concentrate primarily on King as a religious figure with an emphasis on his orthodox religious beliefs and activities. This book challenged my stereotype of King as a political neophyte, totally dependent on the advice and guidance of older, more seasoned black political leaders such as A. Philip Randolph. Although Jackson admits that "King kept his socialism relatively private in a place and time where red baiting and race baiting suppressed movements for basic constitutional rights," he

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carefully documents how King "openly supported leftist causes in the United States" with his "searing indictments of militarism, war, and global economic inequality" (p. 16).

By employing this leftist interpretation of King's civil rights years from 1956 to 1968 as a consistent march toward a democratic socialist position, Jackson revises the traditional story that King became radicalized after 1965 in reaction to social and political conditions in the United States. As the author states, "Most of the terms of Martin Luther King's later radicalism were established by late 1964, however, before Johnson's escalation of the Vietnam War, the Watts riot, the rise of black power, and the 1966 Chicago Freedom Movement" (p. 216). Jackson reiterates the full King biography beginning with his childhood and education. However, he emphasizes King's upbringing as a Christian Socialist by a family committed "to economic reform and religious as well as secular ideals of social obligation to the poorest Americans" (p. 29). Jackson portrays a sober youth without any references to the "playboy" image often attributed to King at this time in his life. Rather, his education at Morehouse College, Crozer Theological Seminary, and Boston University School of Theology exposed King to the language and culture of "Baptist folk preachers, liberal Protestantism, and the democratic left" (p. 36). In fact, it was his exposure to the religious and academic left during his college years (and at the height of the Cold War) where King learned firsthand about socialism from the "survivors of the carnage on the political left": Randolph, Stanley Levison, Bayard Rustin, Ella Baker, Ralph Helstein, and Walter Ruether (p. 40). Jackson argues that the foundational influences on King's later radicalism included black Christianity, the situationist sociology of Walter Chiver, the Social Gospel, Gandhian *satyagraha*, and Marxism.

From this re-evaluation of King's early radical influences, Jackson launches into the well-known story of each of the major campaigns of the modern civil rights movement: the Montgomery Bus Boycott; the formative years of the Southern Christian Leadership Conference (SCLC) in the late 1950s; the symbiotic relationship between King's SCLC and the Student Nonviolent Coordinating Committee (SNCC), the emerging movement for civil rights on major college campuses, whose members shared a commitment to Gandhian nonviolence and direct action tactics; the failed Albany campaign; and the Birmingham campaign that led to the triumphant 1963 March on Washington and ended tragically with "Bombingham," the bombing of Birmingham's Sixteenth Street Baptist Church, a watershed event that usually separates the early successes of the civil rights movement from its violent and tragic end. Yet throughout his narrative, Jackson reiterates the main message of King's deeply held religious and political commitment to social and economic justice for all. Therefore, when Jackson dedicates the second half of his book to King's later political activism, including his growing disagreement with President Lyndon Johnson's War on Poverty and the Vietnam War; the Chicago Freedom Movement; the Poor People's March on Washington; and the final Memphis campaign, the reader better understands how King's commitment to economic justice and human rights came from a "religiously informed Christian socialism . . . considerably evolved in praxis" (p. 362).

Jackson has made a significant contribution to the already extensive scholarship on King, because he recovers a radical voice that has been lost in the culture of celebrity surrounding him. This well-documented and well-written book should interest scholars of African American history, labor and radical history, civil rights and social movement history, and the political and international history of the 1960s. But I think it should be of particular value to scholars of American religious history because of the new light it sheds on the political beliefs of the most famous American religious leader of the twentieth century, and especially to those of us interested in the religious Left.

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JOSEPH CRESPINO. *In Search of Another Country: Mississippi and the Conservative Counterrevolution*. (Politics and Society in Twentieth-Century America.) Princeton, N.J.: Princeton University Press. 2007. Pp. xvii, 360. $35.00.

Joseph Crespino opens his interesting and important monograph on white Mississippians' resistance to the civil rights movement with presidential candidate Ronald Reagan's campaign visit to Neshoba County in 1980, the place where three civil rights activists were famously murdered in 1964. Elucidating the connection between modern conservatives who avow racial equality and the southern segregationists who so strongly resisted it comprises the aim of his book. In doing so, Crespino counters the facile historical claims of conservatives who identify their movement with the religious, nonviolent, and integrationist civil rights crusades of the 1950s and early 1960s. The fight against the civil rights movement in all its phases, he argues, was "at the center of the conservative resurgence since the 1960s" (p. 10).

In making this case, Crespino joins Kevin M. Kruse (*White Flight: Atlanta and the Making of Modern Conservatism* [2005]) and Matthew D. Lassiter (*The Silent Majority: Suburban Politics in the Sunbelt South* [2006]) in placing the politics of moderate, middle-class, often suburban southern whites at the center of post-1960s American political history. These authors illuminate a "revitalized conservative revolution" (p. 82), in Crespino's apt phrase, in the cities and suburbs of the South and West. As African Americans and liberals pushed civil rights and desegregation, southern voters turned to the Republican Party, and southern conservatives easily adapted race-neutral rhetoric of limited government, low taxes, and free-enterprise economics, often withdrawing into suburban enclaves at the same time and advocating the privatization of previously public civic

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functions. Conservatism amounts to an aggressive rearguard campaign in Crespino's formulation, as many white Mississippians went "in search of another country," a conservative, Christian nation they wished to restore to pre-eminence (p. 3). In making these arguments, Crespino, Kruse, and Lassiter join a growing number of scholars of American conservatism who have reoriented our understanding of the emergence and development of postwar American conservatism away from national intellectuals and to the grass roots of American political life—the suburbs, not the cities; the South and West, not the Northeast or Midwest. In focusing on a grass-roots repudiation of liberalism they build on the older "backlash" scholarship exemplified by Jonathan Rieder, Ronald Formisano, and Thomas Sugrue and on a newer wave of studies of the grassroots Right by Lisa McGirr, Rick Perlstein, and Donald Critchlow, among others.

Crespino demonstrates the early and mutual affinity between Mississippi segregationism and right-wing extremism but also how, by 1963, southerners were happy to silence direct appeals for segregation while exporting southern-style racial politics to the rest of the nation. (The Mississippi-based Women for Constitutional Government took care to jettison terms like "states' rights" and "segregation" in favor of bland declarations of conservatism and constitutionalism.) The Mississippi State Sovereignty Commission bankrolled the Coordinating Committee for Fundamental American Freedoms in 1963–1964 (using more than $200,000 donated by a wealthy New York racist named Wickliffe Preston Draper), which fanned the flames of the nascent white backlash movement embodied in segregationist Alabama Governor George C. Wallace's quixotic run for the Democratic presidential nomination in 1964. After Wallace's strong showing in the Maryland primary, this committee took out a full-page ad in the *Washington Post*, crowing: "Since 1932, the Ethnic Groups have been the locked-in difference between Democratic victory and Democratic defeat. . . . As of now—thanks to the Civil Rights Bill—the Democrats have lost this vote. The Democrats have torn loose their political backbone" (p. 96). While Crespino manages to show in greater detail the connection between national conservatives and Mississippi segregationists, it is not clear how important the Mississippians were to the national conservative revolution nor how decisive their contribution was to national shifts in political allegiance.

At the same time, Crespino fleshes out the story of white resistance to desegregation in Mississippi in new and interesting detail, making good use of the files of the Mississippi State Sovereignty Commission to illuminate the deep connection between the Sovereignty Commission and Mississippi's white Citizens' Council and the complex and subtle role the Sovereignty Commission played in desegregation. Crespino stresses the importance and potency of powerful "practical segregationists," such as Senator John Stennis and Governor J. P. Coleman, and later even Sovereignty Commission head Erle Johnston, in blunting the power of the more aggressive and violent white supremacy groups like the Citizens' Council. They saw that "strategic accommodation" to blacks and the federal government rather than violence and defiance would allow Mississippi to prolong segregation and accommodate racial change on its own terms. In addition, Crespino mildly challenges David L. Chappell's account of the role that southern religious congregations played in undercutting massive resistance (*A Stone of Hope: Prophetic Religion and the Death of Jim Crow* [2004]), finding that while Mississippi conservative Christians did recoil from racial extremism, their disdain for liberal Protestant church activists from the North, particularly in the form of the Delta Ministry project, fomented a rising anti-liberal political consciousness.

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Jon C. Teaford. *The Metropolitan Revolution: The Rise of Post-Urban America*. (The Columbia History of Urban Life.) New York: Columbia University Press. 2006. Pp. 306. Cloth $74.50, paper $27.50.

Jon C. Teaford ranks among the most respected, prolific, and widely cited American urban historians of our time. It all began, over thirty years ago, with the publication of *The Municipal Revolution in America: Origins of Modern Urban Government* (1975). His latest book, part of a longstanding urban history series edited by Kenneth T. Jackson, provides readers with an especially well developed set of timely perspectives on the circumstances of the United States from 1945 until the close of the twentieth century.

In a series of seven chapters the author guides readers through a succession of artfully constructed explorations, organized in chronological progression. Teaford stakes his claim by noting that the certainties of postwar America gave way to the ambiguities pervading the 1990s. What he regards, most aptly, as "the new scenario" (p. 7) entails transformations having to do with spatial as well as racial realities that would have defied prediction—and even tested credulity—if used to describe the American nation-state in the immediate postwar era.

A hallmark of Teaford's scholarship is his renown as a researcher and urban historian whose findings are analyzed with a welcomed air of probity. Unquestionably his latest book is no exception. The author delves into a succession of historical issues mostly ignored by his peers. Chapter five, "The New Urban World," provides one example. Among the topics that Teaford plumbs in this chapter is the making of gay ghettos in San Francisco, Chicago, Seattle, and Los Angeles. Admittedly he is not the first urban historian to assay gay culture, but it remains rather uncommon for it to be effectively integrated in considerable detail—as in the discussion of Chicago's annual Gay and Lesbian Pride Day Parade in 1983—within a broader synthesis. Chapter seven, "Beyond the Black-White City," explores the transformed demographics spurred, in considerable part, by the

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landmark Hart-Cellar Immigration Act of 1965. It, too, studies the topic in depth. Teaford's portrayal of the economic niches established by Korean immigrants in California and New York City late in the 1990s is a case in point (they constituted sixty percent of independent produce dealers, fifty percent of manicurists, and some 1,500 dry cleaners).

There are also some lapses in this book worth identifying. The concept of megalopolis—brought to the fore by Jean Gottmann nearly a half-century ago—surely merited discussion. When Gottmann's study appeared his geographic conception—based on mid-century data—encompassed one-fifth of the nation's population or 37 million people arranged within ten contiguous states between New Hampshire and Virginia as well the District of Columbia. Attention to megalopolis would have more fully explained some of the transformations at the center of Teaford's argument. Nor does the discussion of Lyndon B. Johnson and the nation's convulsive cities in the 1960s explore the measurement known as the poverty line. Instantly that concept assumed an enduring back story all its own, spawning a cottage industry that to this day furnishes work for all sorts of bureaucrats, social scientists, and national political figures. Also missing is some discussion of Jimmy Carter's controversial report in 1979 that heralded a prolonged era of diminished federal support for American cities. And while Teaford opted, rather neatly, to close his narrative with the 1990s, this reader yearned for a discussion of 9/11—perhaps it will appear in a revised edition some years hence—as a profound reshaping influence upon the American metropolis.

Setting aside the issues raised in the preceding paragraph, this book demonstrates why we need to have more monographs that take form in the category best classified as contemporary history. To Teaford's credit, he has reached back more than sixty years and then proceeded to move forward chronologically, and self-consciously, to the end of the twentieth century. In so doing, the author has constructed an argument that enables readers early in the twenty-first century better to comprehend circumstances that are very much a part of their recent past, and in some instances their living memory.

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Michael H. Hunt. *The American Ascendancy: How the United States Gained and Wielded Global Dominance*. (H. Eugene and Lillian Youngs Lehman Series.) Chapel Hill: University of North Carolina Press. 2007. Pp. 404. $34.95.

"The United States," writes Michael H. Hunt, "now occupies a global position of unusual, arguably unprecedented dominance" (p. 1). While many Americans celebrate their nation's ascendancy, others worry about its consequences. Few of them, however, understand its origins and subsequent history. They are not helped by policy makers, the press, or other opinion leaders who "have made relatively little use of the past to frame the issues of the day even though history offers precisely the kind of steadying insights needed at a time of confusion and disorientation" (p. 1). Complementing historical amnesia is the truncated standard narrative that foreshortens the trajectory of America's rise to power and fails to link it to massive changes in the nation's relation to the wider world. These are the deficits that Hunt set out to correct in this book. He has succeeded brilliantly.

Most historical narratives of America's rise to power err by locating its origins in the mid-twentieth century. In fact, Hunt shows, the essential elements had been assembled earlier. The first is wealth, a precondition of the material resources on which international power rests. Wealth, in turn, presupposes a commitment to innovation and to developing the science and technology that drive economic growth. Second is "national confidence"—big ideas that result in a sense of national purpose and a widely shared vision that legitimates national sacrifice. Third is a "strong state" that promotes national unity, mobilizes resources, and nurtures leaders marked by "sound instincts and rich experience." By the late nineteenth century, the first two elements were in place; a strong state soon followed.

A more global history of the nation's rise to international power begins by locating America among the "neo-European settler societies" that appeared in the seventeenth and eighteenth centuries and then showing its continued imbrication in a dense network of North Atlantic connections. These international links forced Americans to set aside their suspicions of concentrated political power in order to accommodate a new nationalism that depended on a strong national state, as, between the 1880s and 1940s, "global ambitions came to occupy a position of orthodoxy, in effect becoming the state ideology" (p. 4). As America's global influence diffused through "the creation of an economic and social modernity that has left an ever-deeper imprint on peoples everywhere" (p. 6), the nation realized its worldwide ambitions through cultural as well as by economic and military dominance.

Hunt's analysis of America's ascendance highlights three of his observations about how history works. First is the enduring importance of nation-states in the age of globalization. Instead of eroding the importance of national states, global market forces, contrary to much conventional wisdom, enhanced them. Second is Hunt's rejection of inevitability. Rather than "preordained," America's rise to power revealed the importance of contingency: changing constellations of material circumstances, events, and leadership. The importance of leadership, in turn, points to the third observation: the crucial role played by individuals whose intelligence, judgment, vision, and character shaped national policies. "Structures create preconditions and set limits; individuals have to decide what to do within those limits" (p. 8). The point is not original, but it cannot be stressed too often.

For Hunt, elites make foreign policy. In fact, a rel-

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atively coherent, well-educated, eastern elite directed American national policy from the late nineteenth century through the early post–World War II period. With some exceptions, Hunt admires them and joins other historians who have been rehabilitating the reputations of national leaders of the 1920s. By contrast, his withering criticism devastates the policy of the new elites whose response to the unprecedented challenges arising from Asia, Latin America, and the Middle East has reflected a disastrous brew of staggering ignorance and the blind faith in markets underlying neo-liberalism.

Where does this recent failure of leadership leave America's ascendance? America should be thought of as a "hegemon" rather than an "empire" because the nation's preeminence rested on a combination of "military-political and economic-cultural" power that earned it a far-reaching legitimacy. At its core lay "values shared with other peoples and a confidence in the dominant power's fairness" (p. 312). This has now been squandered, leaving the nation's international position fragile and vulnerable. Hegemony, points out Hunt, entails "a special obligation of stewardship" that America has neglected by its inadequate response to a host of worldwide problems: demographic explosion, massively increased inequality, the "nuclear cloud," and, perhaps most serious, "environmental degradation." Deep suspicion of the United States and opposition to its policies, even among allies, has resulted, further weakening the nation's international position. Nonetheless, isolationism is not an answer. A too rapid or "diminished" U. S. role "could prove dangerously disruptive of the international system and imperil the well-being of peoples everywhere" (p. 320). Instead, Hunt advocates retreating from hegemony toward a "more modest and sustainable" world role characterized by "collaborative internationalism." He is not optimistic that this is the path the nation will follow.

This book combines Hunt's own scholarship with a synthesis of a massive amount of literature. Individual chapters are lightly footnoted with references only to quotations and facts. Chapter by chapter bibliographic essays that follow the footnotes provide an extraordinary guide to the several bodies of scholarship on which the book rests. Hunt writes with clarity and verve; the book's organization is crystal clear. With the 2008 elections approaching, this volume could not be more timely. It should be on the desk of every candidate for national office.

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DANIEL KANSTROOM. *Deportation Nation: Outsiders in American History*. Cambridge, Mass.: Harvard University Press. 2007. Pp. xi, 340. $45.00.

This is an ambitious, thought-provoking study. Daniel Kanstroom approaches familiar laws and court cases with a particular focus, highlighting specific provisions involving deportation and tracing the evolution of the government's sovereign, plenary power to determine how long non-citizens may remain in America and the provisions under which they can be summarily excluded or expelled. The author emphasizes that the extent of the federal government's power to exclude or remove individuals (largely free from judicial review) is dangerous and undesirable.

Kanstroom concludes that American deportation law is arbitrary, indeed often "standardless." At several points, his references to contemporary events intrude into historical accounts, such as erroneously referring to the repatriation of Mexicans during the 1930s as "ethnic cleansing" (p. 219). Kanstroom initially concentrates on recent developments, maintaining that deportation laws were the principal government response to 9/11 (p. 9). However, he largely devotes the final chapter not to analyzing recent events and legislation but to describing various forms of discretion available to those judging deportation cases.

To demonstrate that most deportation proceedings have been placed beyond judicial review, are punitive, and represent a potential threat to citizens, Kanstroom provides a historical examination of the emergence of federal exclusion and deportation controls in immigration and citizenship laws. Contrary to the nation's mythology, he asserts that the United States possesses very arbitrary deportation policies that serve as "a fulcrum on which majoritarian power is brought to bear against" marginalized groups (p. x). Deportation is presented not simply as immigration policy but as "a powerful tool of discretionary social control" (p. 5).

Evolving from colonial laws, England's expulsion of the Acadians, and congressional opposition to England's criminal dumping, the Federalists' Alien Enemy, Alien, and Sedition Acts of 1798 were the first to empower the president to expel undesirable foreigners without jury trials. Subsequent court cases embraced the concept of the government's inherent sovereignty, and, based on that, its right to control who enters and who remains. That perception "serves to this day as the doctrinal substratum for deportation law" (p. 96).

The laws regarding the Chinese expanded the initial concepts, with the power to expel becoming "essentially the same" as the power to exclude (p. 119). A few years later, the 1891 Immigration Act included the first deportation provisions since 1798. Underway was the process by which border control powers were expanded to embrace "post-entry social control deportation" (p. 124), pertaining to actions committed after an individual's admission. The 1901 assassination of President William McKinley prompted legislation in 1903 providing for "ideological exclusion," which was applied during World War I and in response to postwar radical activities, triggering a "deportation delirium" in 1919 (p. 147). Another layer was added when the Asian provisions of the 1917 and 1924 immigration acts "made racial exclusion a central component of twentieth-century immigration, naturalization, and deportation law" (p. 133). Congress also added "retroactive deportation," encompassing actions preceding the authorizing legislation.

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By the 1930s "all of [the] elements characterizing deportation law to this day" (p. 161) were pretty much in place (with important additions in 1950, 1952, 1996, and 2001). Thus, by 1954 the U. S. Supreme Court, in a case involving crime figure Carlos Marcello, could declare that Congress may deport non-citizens "'at any time for any reason even on grounds nonexistent at the time of their entry'" (p. 176).

In the last segment of the book, the progressive evolution of deportation that Kanstroom has laid out gives way to a more piecemeal treatment of various topics. Regarding Japanese Americans during World War II, he refers to "judicial passivity" (p. 213) but says nothing about *Ex Parte Endo* (1944) or the later compensation to survivors. Concerning the repatriation of Mexicans during the early 1930s, he refers to the voluntary departures as "self-deportation" and, as noted, the removal efforts as "ethnic cleansing"—although no comparable murders took place. He labels Mexicans' migrations during the 1950s (both legal and not) as the "greatest peace time invasion ever" (p. 222), a rather inflammatory choice of terms. Kanstroom also comments that the Braceros' numbers were "almost exactly the same as the number of deportees," implying they were one and the same (p. 224).

The final chapter would have contributed more with a closer analysis of post–9/11 round-ups, registration of specific non-citizen populations, and deportations of many Middle Eastern visa violators rather than a discussion of prosecutorial, ultimate, interpretive, and factual interpretive discretion in deportation cases. Nonetheless, although Kanstroom gets a bit preachy at times and inexplicably becomes less sharp in his presentation of events after the 1930s, there is still much that is valuable in this book. He argues that readers need to acknowledge the evolution of arbitrary aspects of American immigration laws. Critical policies and administrative practices emphasizing the deportation option did gradually emerge but were not always as apparent as Kanstroom shows us.

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CARIBBEAN AND LATIN AMERICA

Rosa E. Carrasquillo. *Our Landless Patria: Marginal Citizenship and Race in Caguas, Puerto Rico, 1880–1910.* Lincoln: University of Nebraska Press. 2006. Pp. xxiv, 202. $55.00.

Caguas is a small town in Puerto Rico's heartland, south of the capital, San Juan. Rosa E. Carrasquillo takes the region around this town as point of departure for a micro-historical analysis. On the basis of local documentary sources and some interviews, she focuses on the period when Spanish rule in Puerto Rico was replaced by that of the United States.

After an introductory chapter, the book touches upon a number of themes. First Carrasquillo analyzes the relationship between national and local politics in this period of transition. She demonstrates how the Spanish colonial government embarked on a policy of administrative centralization after the end of the Ten Years' War in Cuba and the subsequent Paz de Zanjón. The Municipal Law of 1878 sought to undermine the power of local elites and, more important, to expand state power in the countryside. Its ultimate goal was control of the rural population. The law fundamentally changed the relation between the central government and local structures of governance and prepared rural society for the push toward capitalist modernization and labor control. Carrasquillo especially focuses on the awkward position of the intermediary officials, *comisarios* (commissaries) and *alcaldes de barrio* (justices of the peace) charged with controlling the rapidly changing relationship between the rural population and the state. These officials were caught in the middle of an increasingly conflictive relationship.

In the following two chapters, Carrasquillo focuses on the crossroads of class and race. The changes in the administrative structure of the country diminished access to land for the rural population. New systems of land tax led to the privatization of land and eventually to increased power for large landowners. Popular forms of access to land became more restricted, and all kinds of means to increase labor control were invented, especially through the infamous *libreta* (passbook) and later the so-called *cédula de vecindad* (identity document). The *cédula* had a double edge: it provided new income for the municipal coffers (in the process, further impoverishing the poorest sectors of society), and it provided employers with a legal instrument to control their laborers.

But this was not only a class issue. In the aftermath of slavery the former slave population, but also the free "people of color," gradually lost access to natural resources. This process began with the so-called Mortgage Law of 1880 but became more visible in the following decades. Carrasquillo cleverly uses the information from census reports to illustrate the daily reality of this process. The new relations of production supported by new laws increasingly drove the ex-slave population from the land and turned them into day laborers.

As in other parts of Latin America, this development was accompanied in Puerto Rico by all kinds of ideological musings in the late nineteenth century, the third element of Carrasquillo's inquiry. Such theorizing tended to show that the social cum racial divide was a legitimate and inevitable element of progress and development. During the U. S. occupation of the island, the racial debate acquired new overtones. Interestingly, the U. S. authorities were somewhat at a loss as to how to classify the Puerto Rican population. Some American officials emphasized the "white" origins of Puerto Rico's rural population. However, they also complained about their "indolence" and targeted them equally with policies of labor discipline.

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This story is rather well known, but the brunt of Carrasquillo's book is to show how the rural population resisted the efforts of increased state control. She uses the concept of "marginal citizenship" to argue that the rural population resisted state measures and legislation and created their own kind of citizenship on the periphery of state power. They disobeyed state rulings and migrated away from abusive landlords or occupied lands. It is surprising that Carrasquillo does not refer to the work of James C. Scott, particularly his ideas about "every-day protest" and "hidden transcripts," because her concept of "marginal citizenship" seems to refer to the same processes that Scott described in the 1980s (and that engendered a massive literature).

The book's last chapter looks at gender relations within rural society. This chapter is quite interesting because of its original source material but stands somewhat apart from the rest of the book. It intends to unravel the logic of patriarchal relations in the countryside. The author shows how prevailing gender stereotypes determined the measures of the state. At the same time, the reproduction of some touching love letters allows her to show how men and women followed their hearts despite all kinds of gendered state repression.

This book is certainly a welcome contribution to Puerto Rican social history and gives interesting insights into the transition period of the late nineteenth century. Its strength is that it tries to present the viewpoints of the poor rural population. However, its origins as a Ph.D. dissertation have not entirely disappeared. The book sometimes lacks authority, and its theoretical basis is somewhat underdeveloped. In addition, the account of the period of the early U. S. occupation is surprisingly weak. This may be the result of Carrasquillo's exclusive use of local sources, which did not highlight the objectives and measures of the new authorities. This leads to my main concern with the book: its exclusive Puerto Rican focus. Although the bibliography presents a wealth of comparative and theoretical literature, the book's perspective is entirely local. Not even Puerto Rico's history is brought to light, nor is the peculiar history of Caguas compared to regions outside of the island. Local history has proven to be a very important part of the historical discipline, but its strength can only be understood when it is placed in a clear comparative framework. Such a framework is missing in this otherwise very interesting study.

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CEDLA, Amsterdam

HARVEY R. NEPTUNE. *Caliban and the Yankees: Trinidad and the United States Occupation*. Chapel Hill: University of North Carolina Press. 2007. Pp. xiii, 274. Cloth $59.95, paper $21.95.

From 1941 to 1947, U. S. military service personnel were stationed in Trinidad. The U. S. military built air and naval bases there under the aegis of the "destroyers for bases" deal (1940) between the United States and the United Kingdom. The mission was to protect the Caribbean region from attacks by Nazi Germany. The deployment of U. S. forces reached 25,000, giving the United States a notable presence in the small island whose population numbered 500,000. Trinidadians have traditionally dubbed the period as the era of "American occupation." As expressed, for example, in the calypso song "Rum and Coca Cola" (1943) sung by Lord Invader, the U. S. presence was perceived as a time of "sexual imperialism." The comparatively wealthy U. S. troops threw their money around, and soon mothers and daughters were "working for the Yankee dollar." The U. S. comedian Morey Amsterdam also acted imperially when he pirated the song and altered the ribald lyrics. The U. S. version of "Rum and Coca Cola" (1944) sung by the Andrews Sisters was an immediate hit, selling 200,000 copies. The song presented a dreamy, tropical vision of international relations. Young girls affirmed that they were treated nice and that the Yankee soldiers "make Trinidad like paradise."

Harvey R. Neptune finds that neither version of the famous calypso song appropriately depicts the wartime experiences of the people of Trinidad. In this delightful cultural history, Neptune suggests that the words "complex" and "ambiguous" best characterize Trinidad's war years. Neptune mined archives in the United Kingdom, the United States, and Trinidad and closely read Trinidad's literary sources. Focusing on the issues of race, class, and gender, the author finds that the U. S. presence in Trinidad did not always negatively influence the British colony's social relations or retard Trinidad's evolution toward independence. Trinidad's patriotic intelligentsia reacted in alarm to the cultural mores of U. S. troops. But Trinidad's subalterns (the poor, women, youth) fashioned something for themselves out of the U. S. presence.

The U. S. military transformed the colony's economy. Over 25,000 Trinidadians worked for the U. S. military. Many more provided services in the informal economy. The poor flocked to work for Americans because of the superior pay and work conditions. The colonial government tried to enforce a "prevailing wages" policy, but the U. S. demand for labor often overwhelmed the British desire to protect colonial employers. Property owners, many of them from the island's tiny white minority, complained that workers had become too demanding. Wealthy homeowners carped about the "servant problem." Trinidad's poor had become conscious of new possibilities in their lives and had begun to undermine the traditional class system.

In the areas of dress, music, and sex, Neptune presents imaginative interpretations. The "zoot suit" became the rage on the island and the local dandies or "Saga Boys" became the island's new cultural arbiters. The U. S.-inspired fashions were rejections of Anglo norms and traditional conceptions of manhood. The embrace of the new fashions, such as untucked shirts, also meant that Trinidadians were choosing clothes

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more suitable for tropical climes than the buttoned-down look of the chilly mother country. Neptune concedes that many of the calypso songs expressed dismay about the easy ways of U. S. troops with Trinidadian women. But the calypso singers composed bawdy lyrics, knowing they would appeal to U. S. soldiers. The singers had earned the Yankee dollar and, with a U. S. audience, greater social acceptability. What especially fascinates Neptune are the open, interracial encounters between U. S. men and Trinidadian women, which he perceives as undermining the island's racial hierarchy. Trinidad's white elite had kept their liaisons with women of color discreet. Anxious colonial elites encouraged Trinidad's young white females to meet and greet U. S. troops at military clubs. U. S. military officers, reflecting U. S. racial norms, often refused to approve marriages between their subordinates and Trinidadian women of color.

Notwithstanding these excellent explorations in cultural history, Neptune leaves some critical areas unexamined. In the 1940s, Indo-Trinidadians, descendants of indentured servants who migrated from India to the Caribbean in the nineteenth century, constituted perhaps thirty-five percent of the population. Neptune repeatedly asserts that in areas of dress, music, and sexuality in wartime Trinidad, "the notion of an ethnic divide was well-nigh negligible" (p. 85). But the voices and views that Neptune cites are almost always those of Afro-Trinidadians.

The embrace by a scholar of the popular term "American occupation" misleads as much as it informs. What Neptune discovers about the response of Trinidadians to the U. S. presence is remarkably different from what other scholars have found in their studies of the U. S. occupations of the Dominican Republic, Haiti, Nicaragua, and Cuba in the first third of the twentieth century. In those cases, the poor and dispossessed resisted U. S. occupations. Perhaps poor Trinidadians made good use of the U. S. presence in their nation because they accepted that the U. S. military's primary mission was to protect them from the Axis powers. Trinidadians further understood that the troops would depart when the war was over.

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Timothy J. Henderson. *A Glorious Defeat: Mexico and Its War with the United States*. New York: Hill and Wang. 2007. Pp. xxi, 216. $25.00.

This book provides a primarily narrative history of the conflict between Mexico and the United States. Beginning with a comparative review of the two nations' history up to 1821, the author then recounts the familiar series of events that began with the colonization of Texas and ended with Mexico's cessation of territory in 1848.

Timothy J. Henderson's text provides a clear example of the difference between writing for an academic audience and writing for the general public. In the former instance, readers expect to find a substantial record of primary as well as secondary sources. For many (including this reviewer), one of the great pleasures of reading a colleague's work is learning of the archival materials used in writing the book. For such readers, this volume will be a disappointment. With one exception, all of the sources cited from the war years appear to have been previously published.

However, books written for the non-specialist need not break new archival ground. Such works must be written in jargon-free language and should possess an internal structure strong enough to tie together various strands from different types of history: social, diplomatic, and military, among others. Viewed in this light, the book under review possesses considerable merit.

Henderson's summary of early Mexican history develops a theme of disunity he then traces throughout the history of the war. Such structure is admirable. Also, he effectively intertwines domestic politics on both sides of the border with international relations to provide a smooth transition for readers who previously approached the topic using a more limited framework.

However, two contentions in particular merit a rebuttal. First, the author states that he "seeks to explain why Mexico went to war with the United States in 1846" (p. xvii). At the end of the book, he offers as explanation a Mexican newspaper's statement that "Defeat and death on the Sabine would be glorious and beautiful" and to be preferred over "infamous and execrable peace" (p. 191). While editorial writers are entitled to use such florid prose, the leaders of any nation that considers itself invaded have little choice other than to resist.

The entrance of General Zachary Taylor's army into the territory between the Nueces and Rio Grande rivers as well as the United States Army camp subsequently established at Brownsville confronted Mexico with the choice of accepting a foreign army's presence on land that the Mexican government regarded as Mexican or of trying to evict that force. Axiomatically, a government unwilling to act in defense of the national territory ceases to be regarded by its citizens or foreigners as sovereign. Mexico went to war because Mexico had no choice. Yet Henderson compares that decision to the suicide of General Manuel de Mier y Terán, concluding: "In a sense, Mexico's statesmen resolved to face war in the same spirit as General Terán had done in 1832 when he dressed in his finery and fell on his sword. They chose to suffer a glorious defeat" (p. 191). Perhaps the two decisions were not comparable.

Terán chose to die. Several of the men who led Mexico in this war chose to survive rather than charge an enemy line. Indeed, General Antonio López de Santa Anna led the Mexican army in leaving the scene of more than one battlefield defeat. At a critical point in the battle for Mexico City, General Juan N. Álvarez Hurtado disobeyed a direct order to charge and thereby preserved his forces for the coming civil conflict.

Another questionable contention is the characteriza-

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tion of U. S. negotiator Nicholas Trist as a man with "enthusiasm for taking over the entire country" (p. 177). There exists evidence to the contrary. Indeed, President James K. Polk criticized his negotiator for his willingness to yield too much to Mexico: "I can never approve a treaty or submit one to the Senate which would dismember the State of Texas, and Mr. Trist's suggestion, if agreed to, would have done this by depriving that State of the country between the Nueces and the Rio Grande" (Allan Nevins, *Polk: The Diary of a President 1845–1849* [1952], p. 270). A subsequent United States Senate report cited Trist as stating: "My object, through out was, not to obtain all I could, but on the contrary to make the treaty as little exacting as possible from Mexico, as was compatible with its being accepted at home" (Robert Drexler, *Guilty of Making Peace: A Biography of Nicholas P. Trist* [1991], p. 129).

To his credit, Henderson has made a number of noteworthy contributions to historical scholarship. One book that he coedited, *The Mexico Reader: History, Culture, and Politics* (2002), stands as a work many teachers deem essential for their courses. The book under review does not fall into that category.

Irving Levinson
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Myrna I. Santiago. *The Ecology of Oil: Environment, Labor, and the Mexican Revolution, 1900–1938.* (Studies in Environment and History.) New York: Cambridge University Press. 2006. Pp. xii, 411. $85.00.

This book describes how the oil business radically transformed the natural and human environment of Mexico's coastal Huasteca region (Veracruz and Tamaulipas) during the first decades of the twentieth century. It is a captivating and important story. As Myrna I. Santiago tells it, the ancient "indigenous ecology" of the native Teenek—rotating *milpa* (cornfield) agriculture, collective land tenure, and a healthy respect for the rainforest—survived the upheavals of the nineteenth century, despite the increasingly aggressive encroachment of cattle-spreading "mestizo and Spanish" landowning elites backed by government policies that pushed land privatization as the key to social progress. The Teenek fiercely defended their communal ecology, and for the most part prevailed. Then came oil, and everything changed. Led by Edward Doheny of the United States and Weetman Pearson of Britain, cunning and greedy foreign capitalists managed in short order to reorganize land tenure, transform land use, and create (mainly out of migrants) an industrial working class. In the process, the rainforest was destroyed, pollution became endemic, the Teenek were largely displaced, and racist social hierarchies came to shape the new division of labor and the patterns of urban growth (e.g., in Tampico). Company surveyors traversed the countryside looking for oil pits, and land agents followed, leasing subsoil rights from the Teenek and other landowners, including *hacendados* (large holders). Money was often incentive enough, but threats or violence were also effective. According to Santiago, by 1922 the companies "had legal control of approximately 46% of the territory" of the Huasteca (p. 70). *Milpas* became oil camps, pipelines crisscrossed the countryside, large refineries and ports were built. A labor force, rigidly divided, was also assembled: supervisors, craftsmen, and drillers from abroad; and large numbers of unskilled and semi-skilled workers, most coming in from other parts of Mexico. This was the new "ecology of oil."

The book chronicles these developments, from the Porfirian "land grabs" through the boom years of the Mexican Revolution and the government's failed attempts to regulate the companies, up to President Lazaro Cárdenas's oil nationalization in 1938. Its main themes are environmental degradation and the struggles of organized labor. This is the first documented study of how Mexico's oil business devastated ecosystems, killed flora and fauna, contaminated water and air, and ushered in new forms of human sickness and death. Santiago provides numerous examples, perhaps none more vivid than the massive, earth-shaking gusher at Dos Bocas, which caught fire and burned for nearly two months in mid-1908, spreading its poisons far and wide. Santiago also outlines the evolution of labor associations in the oil industry, from political clubs, trade unions and refinery-wide unions in the 1910s, through company unions and collective contracts in the 1920s, to the creation of the Sindicato de Trabajadores Petroleros de la República Mexicana in 1935 and the struggle for an industry-wide collective contract that led to the historic expropriation of 1938, the end of an era. The oil workers, she argues, "were directly responsible for the culminating act of the Mexican Revolution" (p. 292). Her labor history, which focuses mainly on conflicts and conditions in and around Tampico, is fairly conventional, concentrating largely on strikes, political ideology (anarcho-syndicalism, revolutionary nationalism), the workers' "culture of defiance," and the role of the state in labor-capital relations.

On the whole, the research contained in this book is quite good, but the analysis is often problematic. Santiago consulted numerous government and labor archives in Mexico, the United States, and Europe, as well as private papers, oral history collections, and a wide array of printed sources. In particular, her contributions to the budding scholarship on environmental change in Mexico deserve notice. But a number of the book's key arguments rest on flawed conceptual and empirical foundations. I will discuss two. First, to define Veracruz's Huasteca in the late nineteenth century as a place divided between conservation-minded communalist indigenes and modernizing Spanish elites misrepresents the region's socioeconomic diversity and obscures the multiplicity of local forces then driving land-tenure change. There were many business-minded *rancheras* here, both indigenous and mestizo; cattle ranching was expanding rapidly, not just among *hacendados*; and exports of timber as well as chicle

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boomed during the 1880s and 1890s. This is when deforestation began in earnest. Radical changes in property rights, land use, and social relations more generally—in villages and elsewhere—were fast eroding the old "indigenous ecology," and were well underway by the time the foreign oilmen entered the picture. Absent this context, it is difficult to explain, for instance, why the oil companies' land-leasing drive faced relatively little resistance. Santiago notes this, but her framework of analysis cannot make sense of it. Likewise, her story of the labor movement is built on the assumption of a more or less inherent worker solidarity. For the most part, oil workers here speak with a single voice, as if they all recognized *ab initio* that their interests and goals were fundamentally the same. There is little here about the politics and mechanics of labor organization in oil, and thus many central aspects of this history—e.g., "white" unions, keeping the Confederación Regional Obrera Mexicana at bay, the influence of Emilio Portes Gil—are largely glossed over. Despite problems such as these, there is much of great value in Santiago's book. But a broader conceptualization of the historical questions would have produced more compelling arguments.

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Anne-Emanuelle Birn. *Marriage of Convenience: Rockefeller International Health and Revolutionary Mexico.* (Rochester Studies in Medical History.) Rochester, N.Y.: University of Rochester Press. 2006. Pp. xi, 434. $95.00.

In this sophisticated account of seduction, romance, marriage, and separation—international health-style—Anne-Emanuelle Birn delivers a nuanced analysis of the relationship between the Rockefeller Foundation's International Health Division and the revolutionary Mexican government's Departamento de Salubridad Pública over a thirty-year period. Arguing that the New York-based foundation started by capitalist John D. Rockefeller actively "courted" the Mexican government in seeking a venue for an experimental yellow fever campaign in the 1920s, Birn shows that the "marriage" that followed was characterized by competing interests, compromise, benefits sharing, and, when things got rough, occasional therapeutic intervention by the U.S. State Department to keep the relationship intact. While the Rockefeller Foundation secured in Mexico a place in which to test new technical approaches to disease eradication and to strengthen the U.S.-Mexico relationship through study tours and training, Mexico gained access to laboratory resources, professional networks, and international recognition of the revolutionary government's accomplishments in the area of public health. Even as the marriage stagnated and the foundation's interest in health gave way to a new attraction to agriculture and crop experimentation, the principles on which the Rockefeller-Mexico relationship were founded, Birn argues, served to undergird the development of the broader field of international health, which today includes dozens of multilateral agencies, non-governmental organizations, private charities, and bilateral initiatives. Scholars interested in revolutionary Mexico, the development of the field of international health, and the history of U.S.-Mexican relations will find this study essential reading.

The book is based on an impressive array of documents culled from archival collections in Mexico and the United States. Examining information ranging from reports prepared by Rockefeller Foundation staff and institutional budgets to graphic images in health education pamphlets disseminated in Mexico, Birn casts a critical eye on the relationship designed to improve human well-being and modernize society. She identifies five themes that characterized the foundation's work south of the border: agenda setting from above, budget incentives, reliance on a techno-biological paradigm, *a priori* parameters of success, and the generation of consensus via transnational professionals. Over five intricate chapters she reveals the International Health Division's goals to have been directed toward securing long-term relationships with key public health officials; elevating the capacity for public health work in Mexico; and ensuring the foundation's legacy south of the border. When revolutionary public health programs—such as collectivization of land into *ejidos* and the establishment of ejidal health outposts—clashed with the Rockefeller Foundation's position that health was a technical issue rather than a political one, foundation officers generally held their tongues and avoided criticizing the government, redoubling their efforts on disease-specific campaigns they could control instead. Birn also argues that the personalities of John D. Rockefeller and radical Mexican president Lázaro Cárdenas del Río were critical in shaping the outcome and influence of the foundation's Mexico campaigns, including those related to yellow fever and hookworm; strengthening local services; and improving technical education. Using the themes of courtship, partnering, and separation to structure her analysis, she concludes that "the marriage between the RF and Mexico was thus elastic and mutually beneficial, even as the identity of each was at times subordinate to the relationship itself" (p. 261).

Birn expertly weaves the story of public health in Mexico, and the role played by the Rockefeller Foundation in shaping it, into the larger history of revolutionary Mexican politics and reform efforts. She carefully details the institutional histories of the Departamento de Salubridad Pública as well as the foundation's International Health Division, providing considerable background information on key documents and legal frameworks as well as the personalities and political commitments on both sides that shaped the relationship. Despite the density of information provided, Birn's analysis is always focused, and she consistently shows the reader the connections between high politics and the day-to-day undertaking of public health. Her analysis of the impact of the Cárdenas de-

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cision to nationalize the oil companies, including Rockefeller's Standard Oil, is particularly well done. Charting the history of conflicts over foreign companies' oil holdings in Mexico, Birn shows that the Cárdenas administration's decision to nationalize private property in 1938 had a significant impact on the business of public health; as U.S. companies refused to do business with Mexico while the conflict was sorted out at the diplomatic level, Mexico began selling oil to Nazi Germany, and Mexican clinics and hospitals turned to German firms for their laboratory and pharmaceutical needs. The analysis regularly delivers a compelling account of the ways in which institutions, officials, and staff considered the relationship between the Rockefeller Foundation and Mexican government officials. However, while Birn does pepper the analysis with references to union complaints about health programs or communities' rejection of certain procedures, overall she offers very little regarding the ways in which the Mexican people themselves perceived the relationship or role of the foundation in their lives.

Birn ends the book with an interesting epilogue that considers the ways in which the Mexico–Rockefeller Foundation relationship set the stage for the development of the international health field in the latter half of the twentieth century. It is here that she presents the five principles that shaped that relationship and that, she argues, remain relevant today. While she does occasionally refer to the foundation's health-related efforts in other world regions—specifically Brazil and southern Europe—the argument would be stronger if she could show that the five principles had shaped foundation-state relationships in other countries. In the epilogue Birn examines the impact of the foundation's work on the formation of and challenges faced by the World Health Organization. She also suggests that many donor-driven international health efforts, such as the 1962–1980 smallpox eradication program, forced developing countries to invest scarce resources in the eradication of diseases of concern to developed countries at the expense of strengthening their own health systems. Describing new actors in the field, including the Bill and Melinda Gates Foundation, private-sector corporate responsibility schemes, and non-governmental organizations, she notes that "international health's bureaucratic and ideological imperatives continue to keep the five RF principles in play" (p. 283). Nevertheless, the role of civil society in countering donor-driven imperatives and setting alternative health priorities offers hope for Birn, who concludes her analysis with a consideration of how community-driven initiatives might reshape the field of international health. With fewer health programs tied to budget incentives or strings-attached accounting, she notes, community-driven approaches may model a true marriage—instead of a convenient one—between national governments and donors in the next generation.

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THERESA ALFARO-VELCAMP. *So Far from Allah, So Close to Mexico: Middle Eastern Immigrants in Modern Mexico*. Austin: University of Texas Press. 2007. Pp. xi, 272. Cloth $60.00, paper $24.95.

Theresa Alfaro-Velcamp's study of Middle Eastern immigrants joins a burgeoning literature on the role of foreign communities ("colonies") in the creation of modern Mexico. Questions relating to *Mexicanidad* (Mexican identity) are central as she seeks to "reveal [the existence of] a Mexican multiculturalism by examining how Middle Eastern immigrants and their descendents have become Mexican citizens and how they have created their own Lebaneseness" (p 162).

Most Middle Easterners who arrived during the *Porfiriato* (era of Porfirio Díaz, 1876–1911) saw Mexico not as their final destination but as a back door to the United States. After 1891, when the United States tightened immigrant health inspections, smuggling rings proliferated on the border, where they prepared illegals to cross, treating obvious diseases like trachoma and "coaching" them on how to avoid customs inspectors' attention by "acting Mexican" (p. 33). Although Washington pressured Mexico to exclude those deemed undesirable, the Díaz regime, which predicated its modernization program on attracting foreign immigrants as well as foreign capital, balked.

Those who settled in Mexico often were aided by kinsmen who financed travel and found them employment, frequently as shopkeepers or peddlers selling "fine and cheap articles" to the less affluent on installments (*albonos*) (p. 133). New arrivals also received assistance from established businessmen such as Domingo Kuri, known as the "*consul honorario*" of Veracruz, who "played a pivotal role in connecting members of the Middle Eastern community throughout Mexico" (pp. 54–55). When Mexico's 1910 Revolution came, Middle Easterners had little choice but to tough it out. But, as Alfaro-Velcamp notes, the chaos also provided "opportunity to prosper and create their own unique community in the post-revolutionary nation . . . and successfully position themselves in Mexican society" (p. 29). One such entrepreneur who sold rations, arms, and ammunition to revolutionary forces near Torreón was Alfaro-Velcamp's great-grandfather, Antonio Aychur Itt, whose story inspired her research.

With the postwar division of the Ottoman Empire into mandates, kingdoms, and nations, the number of immigrants spiked. Families reunited and Middle Easterners came to regard their residence in Mexico as permanent, not a "sojourn." As they shed expectations of return, they also traded "*turco*" and "*árabe*" identities for an invented Lebanese identity, rooted in an imagined Phoenician heritage and founded organizations (Liga Libanesa, Unión Libanesa), schools (Academy of Arabic Language), and journals (*Emir*, *Líbano*) (pp. 19–24, 43–69, 142–151). Simultaneously, as Mexico's revolutionary violence subsided, its xenophobia crested. Under President Plutarco Elías Calles, said to be of Middle Eastern descent, the government forbade

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immigration by Middle Easterners (now characterized as "parasites") and mandated their registration as a necessary first step to citizenship (p. 84). Thus "inadvertently, policy makers perpetuated the 'foreign citizen' paradigm whereby outsiders maintained multiple identities while joining the Mexican nation . . . acculturating in multiple ways depending on their economic and social positions" (pp. 132, 160). Generally the middle and lower classes assumed a Mexican identity while the wealthy identified themselves as Middle Eastern. For most endogamy was the rule, however; no more than thirteen percent married Mexicans. Nonetheless, succeeding generations were culturally Mexicanized, which broadened their opportunities. Thus, while the Lebanese continued their commercial traditions, exemplified by Carlos Slim Helú whose Telemex holdings are one quarter of Mexico's stock market value, some also managed successful careers in notoriously cliquish Mexican politics (pp. 23, 160).

Although Alfaro-Velcamp did prodigious research in Mexico, the United States, France, and Lebanon, she presents it poorly. The problems are many and varied but most flow from her troubled prose and her substitution of redundancy for analysis. This is seen in her narrative rendering of fourteen tables of data gathered from over 8,000 immigration cards at Mexico's National Registry of Foreigners. For example, she writes of a change in the immigrant profile "from mostly married immigrants (64 percent) from 1910 through 1919 to more single immigrants (40 percent) in the 1920s" (p. 97). Her awkward comparison, which omits the number of widows, hides the actual increase of fourteen percent in the unmarried she shows in Table 9. Her data also suggests her use of the term "Middle Eastern immigrants" is obscurant. The Ottoman Empire and its successors were ethnically and religiously diverse, true, but the population that migrated to Mexico was far less so. Averaged over the period, seventy-nine percent were Lebanese or Syrian; Muslims were a distinct minority at 3.8 percent (pp. 166, Table 2, and p. 170, Table 6). A forthright handling of this would have made her Lebanese turn less abrupt. Alfaro-Velcamp claims her book "is comprehensive with respect to most of the primary and secondary sources" (p. 7), yet she omits significant works on Mexican diasporas, most notably Jürgen Bucheneau's *Tools of Progress: A German Merchant Family in Mexico City, 1865–Present* (2004). Finally, she writes of her intention to "search for inferences from hidden transcripts," yet, sadly, she makes almost no use of the Lebanese-Mexican journals, *Emir* and *Líbano*, where such transcripts likely are hidden (p. 5). In short, Alfaro-Velcamp has written a flawed but useful book on an important topic which I hope she will revisit.

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Jordana Dym. *From Sovereign Villages to National States: City, State, and Federation in Central America, 1759–1839*. Albuquerque: University of New Mexico Press. 2006. Pp. xxxi, 390. $45.00.

A recent and welcome development in the conceptual framework of Latin American history has been the emergence of the eponymous "Middle Period" (circa 1750–1850). After more than a century of viewing the region's history as a stark dichotomy, divided at early nineteenth-century independence, scholars have begun to document to great effect the multiple continuities linking the colonial to the national periods. At the same time, the "Age of Independence" (1808–1826) itself has also come under fruitful review. Long a stage reserved for heroes and patriots, kings and generals, current research has introduced new actors by investigating the diverse roles played by the non-elite majority, including indigenous groups, Africans, and women. In this provocative, carefully argued, and meticulously researched work, Jordana Dym makes an original contribution to both these recent historiographical trends by spotlighting the *municipio* (municipality) and confirming its place at the heart of Central American political life from the beginning of the colonial period, through independence, and into the nineteenth century.

Historians of colonial Spanish America should find nothing surprising about that statement. The importance of the city and the urban experience in the Hispanic world is well documented. Dym, however, does not simply describe the urban environment or examine in isolation the operations of city government. Instead, these communities or pueblos emerge here as full-fledged participants in the turbulent political evolution of the isthmus, a history that, absent their unique contributions as autonomous actors, Dym considers profoundly incomplete. As the fundamental political unit of Central America, the *municipio* was at the center of the revolution in governance that began under the Bourbon crown in the middle of the eighteenth century and continued in radical, unexpected directions through a constitutional experiment and into the nation-building process of the 1800s. The pueblos, she argues, exercised significant influence over the political development of the region during this period in two ways. Their historic role as the locus of citizenship and their ability to wield influence beyond a narrow local setting allowed cities to operate on a surprisingly equal level with provincial, state, national, or imperial institutions and officials. Thus, the centrifugal force they exerted kept the colonial Kingdom of Guatemala decentralized, left the post-independence states weak, and promoted the collapse of the Central American Federation. At the same time, decades of municipal conflict combined with the diffusion of municipal governance among heretofore disenfranchised populations, combined with a parallel yet gradual shift in the locus of citizenship to the nation, or pueblo, reinforced the need for a viable central authority. This centripetal force helped establish the foundations for the modern nation-states that now dominate the isthmus.

The monograph is divided into three sections. The first, "The Pueblos Formed," covers the period from 1524 to 1808 and establishes the proposition that municipal government was an "equal partner" (p. 31) with

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the royal officials and the church in the delicate balance of power created by the Habsburgs. Later, during the time of the centralizing Bourbons, Dym concludes that the crown's obsession with uniformity of governance resulted in the creation of more *cabildos* (town councils), thereby diffusing the power of the original "conquest cabildos." These reforms ostensibly favored new provincial intendancies to the detriment of local governance, but the *municipio* emerged as a more "representative" (p. 48) institution by expanding its advocacy role *vis à vis* the crown. Part two, "The Pueblos Restructured," addresses the trauma and opportunity of the Imperial Crisis (1808–1821), when dormant theories of municipal sovereignty reemerged in the absence of the monarch. As Dym shows in great detail, so thoroughly did the municipalities monopolize the political discourse of the time, whether through expressions of loyalty, demands for autonomy, or cries for independence, that one must look to the city councils for the Central American equivalent to a Miguel Hidalgo or a Simón Bolívar. Significantly, the enlightened constitutional reforms of the period mandated another municipal expansion by requiring the establishment of elected councils in all communities with at least 1,000 residents. Dym demonstrates how this experiment, despite significant elite doubt, took hold in indigenous communities and furthered greater acceptance of ideas of equality, popular sovereignty, and universal male suffrage.

The book's final section, "The Pueblos Unleashed," illuminates the destructive and constructive role played by the cities as Central Americans struggled to construct viable sovereignties and institutions on state and national levels after independence. The two decades at the heart of this section (1821–1839) have long been popular with scholars of the region, since they serve as the crucible, for better or worse, for the modern political framework of the isthmus. Dym, however, eschews most of the traditional vocabulary when discussing this period. Absent or minimized are the Liberal/Conservative debates, the Centralist/Federalist divide, state rivalries, individual founding fathers, or disruptive *caudillos*. Instead, to great effect the protagonists remain the cities.

This perspective is an important corrective to the historiography of Central America's Middle Period and should encourage comparable lines of inquiry across Spanish America, not to mention comparative studies in other parts of the revolutionary Atlantic World. A model of contemporary archival research, Dym's bookis almost encyclopedic in presentation, with enough illustrations, appendixes, and figures to serve as a reference source for the period. Students and scholars whose interests take them well beyond the narrow borders of Central America should find much to reflect on in this work.

Timothy Hawkins
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David T. Garrett. *Shadows of Empire: The Indian Nobility of Cusco, 1750–1825.* (Cambridge Latin American Studies.) New York: Cambridge University Press. 2005. Pp. xviii, 300. $75.00.

The post-conquest experience of the Inca nobility of Cusco, the heartland of the Inca empire, was for long overlooked by most historians. In the past decade, however, a spate of studies has explored this theme in depth and restored it to its due place at the forefront of the research agenda. Recent studies have focused mainly on the late colonial era, for which archival materials are abundant and generalizations can be made with some authority. The most significant study to date is David T. Garrett's fine book, which answers some questions while posing others. The conclusions he draws from his data-rich research are always defensible; any residual doubts concern the degree of change or stasis rather than his broader findings.

While focusing primarily on the late colonial era, Garrett's study nevertheless devotes significant space to the seventeenth century; he accepts the consensus that it was an age of transition between the immediate post-conquest status quo and the implementation of the Bourbon version of modernity. Fortunately, he avoids the temptation to focus solely on the Inca nobility, employing it as a lens through which to view the colonial vicissitudes of the wider indigenous society. Most valuable is his treatment of the interwoven fortunes of the nobles and the caciques, a social group whose brokerage function was crucial to colonial governance and economy. The cacique group included some, but by no means most, Inca nobles. It is also of interest that Garrett compares the nobility's lot with that of the Creole (American Spaniard) aristocracy, such as it was. The two groups were closely linked by intermarriage and mutual economic interests, which were to some degree irreconcilable with those of the numerous indigenous communities of the southern sierra. Notary records of the business transactions of both Creole and Inca elites leave no doubt as to the extent of their interlocking interests. In the Great Andean Rebellion of 1780–1782, the Inca nobility, with few exceptions, remained true to the crown, although this was partly a response to the fact that they saw the rebel leader as a usurper of their prerogatives whose self-proclaimed Inca status was highly dubious. Garrett rightly pays attention to the divergent status and wealth among the nobility, which ranged from great prosperity (in the local context) to real hardship. The only hostage to fortune he offers is his curious insistence that a bourgeoisie existed in late colonial Cusco.

It can scarcely be doubted that the Inca nobility was an anachronistic corps whose pre-conquest *raison d'être* was largely expunged by the Spanish conquest; its very existence was snuffed out by liberal decrees soon after the coming of full independence from Spanish rule in 1824. The nobility's elite counterparts, the indigenous caciques, were also abolished as a discrete social group *de jure*, but their official demise is more open to doubt than the author allows. Decades after independence, communities continued to refer, in legal documents, to

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their own authorities as "caciques." Social changes in the parishes of the city and its immediate environs were not necessarily replicated in the provinces, at least not wholly. As ever, there was a gap between promulgation and implementation of the law, and between city and country. A closer look at Roger Rasnake's study of long-term Bolivian indigenous governance might have served to dilute Garrett's view of the abrupt disappearance of the caciques. He is, however, right to avoid casting late colonial changes in the system of indigenous government—especially the incursions of non-indigenous middlemen into the cacical offices—as a neat replacement of *cacicazgos* by indigenous municipalities; different social processes occurring simultaneously often produced contradictory outcomes.

It may be doubted that the Inca nobility was weaker after the rebellion of 1780–1782 than before; there is not much evidence to that effect. The comparison is perhaps somewhat forced: in effect, the nobility as such was never very influential, its prestige deriving more from its prominence, splendidly arrayed in Inca costume and blazonry, on public occasions. However, in the elections attendant on the constitutional crisis of 1812–1814, the Inca nobles comprised some forty percent of electors and delegates in a severely limited franchise. This sits oddly with the author's contention that it was almost entirely moribund after 1780; clearly, the Inca nobility retained some political clout in the parishes of the city and its purlieus. Garrett is assuredly right, however, in his view that the nobility "collapsed" entirely soon after independence, notwithstanding that individual nobles and their families continued to prosper throughout the early decades of republican rule.

This is a fine-grained study, meticulously researched and scrupulously written, and it represents a considerable achievement. It should remain the standard reference on the late colonial vicissitudes of the Inca nobles and other indigenous elites in southern Peru. Its emphasis on social divergence within interlocking indigenous, and even non-indigenous, elites and local governance has significant implications for the study of politics, society, and power well beyond the colonial era.

David Cahill
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Anne S. Macpherson. *From Colony to Nation: Women Activists and the Gendering of Politics in Belize, 1912–1982.* (Engendering Latin America.) Lincoln: University of Nebraska Press. 2007. Pp. xviii, 385. $55.00.

Formerly the colony of British Honduras, Belize is a cultural and political anomaly in Central America. Colonized by English pirates, African slaves, a second Afro-Caribbean population (the Garifuna), and mestizo and Maya refugees from elsewhere in Central America, Belize evidences extraordinary ethnic diversity. British colonialism bequeathed to the now-independent country a parliamentary system after Westminster precedents as well as an official, if in many locales negligible, anglophone identity. Through most of the twentieth century British colonialism was also responsible for a complacent historiography that drew heavily on the self-serving rationales of colonial administrators. Beginning with archival excerpts published in the 1930s by Governor Sir John Burdon and continuing through the final decades of colonialism, an officially sanctioned history cast the colony as a bastion of freedom in a region of autocracy supposedly abetted by Spain, local *caciques*, and the Catholic Church. These accounts leaned heavily on the colony's unofficial creation myth, the 1798 Battle of St. George's Caye, in which African slaves and their English masters were said to have fought "shoulder to shoulder" to fend off Spanish incursion into the territory. As late as 1961, a comprehensive history of the colony asserted that slavery "was much less oppressive than elsewhere" (D. A. G. Waddell, *British Honduras: A Historical and Contemporary Survey*, p. 14), a claim harkening to the apologetics of nineteenth-century slaveowners.

It was left to later scholars to reveal the class and ethnic conflicts effaced from such accounts. O. Nigel Bolland identified the ways in which a "forestocracy" of timber companies and merchants extended the servitude of forest workers long beyond slavery's formal abolition in 1838. During most of the colony's history, elites monopolized vast tracts of land for timber production, denying its use for farming in order to control the colony's sparse working population. Peter Ashdown added nuance to this picture by examining the acrimonious relations between the occasional reform-minded governor and the colony's largest landowner, Belize Estate and Produce Company (BEC). And in making much of this material available to Belizeans themselves, Assad Shoman strengthened both the nationalist movement and created a "people's history" that now informs the curricula of all the country's secondary schools.

Conspicuously absent from such accounts, except when it is treated as the epiphenomenon of a broader class struggle, is the role of women in Belizean political history. Anne S. Macpherson succeeds brilliantly in remedying this omission. Her book is the first significantly new contribution to Belizean historiography in decades. Much as Bolland, Ashdown, and Shoman overturned an earlier model of official history, Macpherson has both built upon and revised interpretations received from them. Belizean women emerge in her account as central political actors in their own right, often taking up mobilizations abandoned by male workers or sustaining popular movements when male leadership was timid, compliant, or divided.

Macpherson's revisionism begins with the very event marking the emergence of Belizean nationalist politics, the 1919 "ex-servicemen's riot," in which returning World War I veterans protested the lack of colonial land grants and work for discharged soldiers. Their actions escalated into a night of generalized rioting that terrified the administration and white elites, leading the state to adopt a series of tepid reforms over the next decade to quell nationalist sentiments. While previous

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historians viewed the riot narrowly as the expression of veterans' grievances, Macpherson demonstrates that women's concerns with wartime inflation and starvation wages weighed just as heavily in the action. In the ensuing years, women became both the objects of reform, in the case of working-class and rural women targeted for "improvement," and, among middle-class Creoles, allies and promoters of the state's reform project. Unlike Ashdown, who traces the origins of the independence movement to adherents of Marcus Garvey's United Negro Improvement Association (UNIA), Macpherson argues that middle-class Garveyites actively aided the government's reform strategies, placing them at odds with the overtly nationalist Labourers and Unemployed Association (LUA). Unlike earlier reformist organizations, the LUA under Antonio Soberanis granted full membership to women, a gesture that unleashed female participation in strikes and other protests leading eventually to the recognition of labor unions by a reluctant colonial administration. In one landmark action, a 1934 strike against the BEC sawmill, women seized the initiative after male workers fell back, forcing their way through the factory gate armed with "sticks and kitchen utensils, potspoons and kitchen forks," in the words of one contemporary (p. 145).

In Macpherson's account activist women emerge as complex strategists, seeking alliances with male leaders to advance their interests as workers and mothers in tandem with the causes of universal suffrage and then national independence. Macpherson draws heavily on archival records and interviews with more than 100 participants in the colony's mass mobilizations from the 1910s onward. Absent from the country's history books until now, the elderly women featured on these pages speak powerfully of the sense of agency—indeed, heroism—that motivated them during Belize's fight against British rule. One informant, in recounting her youthful confrontation with a British governor, declared, "I was never a coward woman" (p. 285). Macpherson's history accords such steadfast determination a central role in Belize's emergence as an independent nation, and does so with meticulous research and profound empathy for her subjects.

MARK MOBERG
University of South Alabama

RONN F. PINEO. *Ecuador and the United States: Useful Strangers*. (The United States and the Americas.) Athens: University of Georgia Press. 2007. Pp. xiii, 260. $24.95.

Scholars and activists commonly portray the United States as a hegemonic force that dominates Latin America. Ronn F. Pineo, however, paints a much more complex picture of negotiated relationships between a global superpower and the small South American country of Ecuador. Pineo makes the astute observation that while the United States has rarely given much thought to Ecuador, in contrast the United States has always mattered a great deal to Ecuador. As a result, Ecuador has had the space and motivation to maneuver geographic distance and a lack of U. S. interest to its own benefit.

Pineo organizes his discussion chronologically from Ecuador's independence in 1830 to the present. For much of the nineteenth century, Ecuador remained isolated and on the periphery of global concerns. This was partially a consequence of its weak and fragmented state structures, which led to a great deal of political instability. Ecuador initially had little diplomatic contact with the United States, relying instead on Chile and Mexico to represent its interests. Racism and ignorance characterized U. S. attitudes to Ecuador. Like other Latin American countries, Ecuador looked toward Europe, with elites holding a particular interest in its former colonial overlord Spain, the emerging British empire, and French cultural influences.

The United States began to play a larger role in Ecuador toward the end of the nineteenth century because of growing cacao exports. In addition, the government brought in a U. S. firm to build a railroad from the coastal port of Guayaquil to the capital city of Quito in the highlands with the hope that this would economically develop the country. Deadly disease outbreaks in Guayaquil—an issue that Pineo discussed in his previous book, *Social and Economic Reform in Ecuador: Life and Work in Guayaquil* (1996)—slowed U. S. intervention in the country.

In the 1920s, the cacao economy collapsed and the government of Ecuador invited Edwin Kemmerer, a professor of economics at Princeton University, to remake its banking system. Although his policy suggestions foreshadowed later International Monetary Fund (IMF) policies, Pineo stresses that it is a mistake to see the Kemmerer Mission as an example of economic imperialism. Isidro Ayora invited him to the country, and Kemmerer served at his pleasure. Instead, as Pineo effectively demonstrates, an elite government favored and implemented economic policies that often paralleled the interests of the United States while hurting Ecuador's impoverished and dispossessed population. As Pineo explains, elites from Ecuador and the United States came from the same social class and often shared the same economic goals and aspirations.

Ecuador's independence from U. S. foreign policy objectives also becomes apparent in an examination of military bases on the Galapagos Islands and the Salinas Peninsula. Pineo traces U. S. interest in the Galapagos back to 1853. At times, Ecuadorian elites courted the United States for economic reasons. At other times, nationalistic impulses led Ecuador to reject U. S. overtures. In the end, Ecuador usually managed to have its way in regard to the degree and nature of United States presence in the country. Most recently, in 1999 President Jamil Mahuad convinced the United States to lease the Manta Air Base for a period of ten years, allegedly to interdict drug trafficking in neighboring Colombia. Current President Rafael Correa, however, has refused to renew that lease. In both cases, Ecuador had

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as much control over the bilateral relations as did the United States.

Ecuador also exhibited control over its foreign policy decisions in its relations with its neighbors. Ecuador has had long-running border disputes with Peru, and Pineo illustrates how the Ecuadoran government effectively manipulated United States Cold War paranoia to defend itself from what it perceived to be its primary threat and enemy. Similarly, in the 1980s, under conservative President León Febres Cordero, Ecuador was the only Latin American country to break relations with the leftist Sandinista government in Nicaragua. Rather than this resulting from U. S. pressure, however, Febres Cordero took this action in response to what he interpreted as an insult from Nicaraguan president Daniel Ortega. Examining Ecuador's motivations is essential to a proper understanding of these actions.

Although purportedly this book is about bilateral relations between the United States and Ecuador, Pineo positions Ecuador in a much broader multilateral context of political, economic, and cultural relations with neighboring countries and Europe. Although not intended as a comprehensive survey, the book's broad sweep makes it a very useful introduction for any study of modern Ecuador. Pineo has written an important work that will be of interest to specialists as well as to a general readership.

MARC BECKER
Truman State University

FRANK O. MORA and JERRY W. COONEY. *Paraguay and the United States: Distant Allies*. (The United States and the Americas.) Athens: University of Georgia Press. 2007. Pp. xiv, 333. Cloth $59.95, paper $24.95.

This book is a long-awaited addition to the literature about an understudied country at the heart of Latin America. Frank O. Mora, a specialist in international studies, and Jerry W. Cooney, a historian, study the developing dialectic between the nations since Paraguay's independence. Their argument is that relations with the United States varied over time depending on U. S. attention to the Southern Cone and Paraguayan desires to capitalize on its northern neighbor's interest in the region. International events affected relations between these nations, but circumstances as seemingly insignificant as diplomats and their behaviors also made their interaction fluctuate. This book is packed with details that have previously not been readily available; despite having a specific focus, it informs continental developments. Scholars of political science, history, and Latin American studies will find it a useful teaching tool.

At first glance, one might expect the relationship between Paraguay and the United States to have been one-sided, given the disproportionate power balance. Diplomats altered the relationship. Edward Hopkins, U. S. representative in the Southern Cone in 1845, mixed diplomacy with personal gain at a time when U. S. foreign policy was linked to commercial interests. Paraguay tried unsuccessfully to secure U. S. support during its war with Brazil and Argentina (1865–1869), and its disastrous defeat led to foreign speculation through ranching. By 1900, the United States sought to use Paraguay as a buffer state between Brazil and Argentina. American speculator Percival Farquhar built a syndicate of railroads, ranches, and packing companies to cut a rail route from Brazil through Paraguay to the Chilean coast. After purchasing a controlling interest in Paraguay's railways in 1910, Farquhar supported the Radical faction that took power in 1912 after a bloody revolt. The World War I economic slump ended the rail attempt and allowed Argentine and U. S. cattle barons to claim much of Paraguay's land for ranching.

The first career U. S. diplomat in Asunción was Post Wheeler, who arrived in 1929 and favored his host country during deteriorating relations between Paraguay and Bolivia over the Chaco territory. Of particular interest is the authors' analysis of the Chaco War of 1932 and World War II. The role Argentina played in the precursors to the Chaco conflict has been documented, but this study makes U. S. influence throughout evident in a new way. The authors convincingly dispel the myth that oil speculation fueled the war. They do show, however, that Argentina favored Paraguay during the conflict because of its ranching interests, and that despite significant disagreement with Argentine minister Carlos Saavedra Lamas, the United States supported the final 1938 treaty. The Chaco War made Paraguay a factor, albeit minor, for the United States to consider when making hemispheric policy over the next decades, and the 1939 economic agreement with Asunción solidified the new ties. Paraguayan support for the Axis powers threatened the United States, however, with colonies of German immigrants in the Chaco, pro-Nazi newspapers and radio stations, and a Vichy military mission in Asunción. With aid programs, FDR purchased and retained Paraguayan support. From 1943 on, Paraguay traded subordination to Washington's interests in exchange for U. S. aid.

Relations between the distant allies improved when General Alfredo Stroessner assumed power in 1954. The U. S. military, especially, courted the general as the Cold War intensified. In return for aid and a facade of legitimacy, the dictator cooperated with the United States. Both nations agreed that political stability was linked to economic prosperity, and for the dictatorship's first seven years, Paraguay was one of the top U. S. aid recipients in Latin America. The authors argue that without Washington's political, economic, and military support, it is difficult to imagine how the regime could have consolidated its power or even survived. The dictator's loyalty, however, was not blind; Stroessner strategically threatened to turn to Moscow and employed subtle extortion to increase the flow of funds from the United States.

Also clear to readers will be the pressure that Henry Kissinger and Jimmy Carter put on Stroessner to move toward a democratic opening, a largely cost-free decision at the time due to negligible U. S. investments and the low level of threat from the Left. Ambassador Rob-

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ert White pushed hard to improve human rights, and by 1980 the regime was internationally isolated. Analysis of the way Paraguayan society mobilized to oppose the dictator would complement the book's argument, although strictly speaking this was not directly related to U. S. influence. President Ronald Reagan's diplomat, Clyde D. Taylor, had the greatest impact on Paraguay's democratization in 1985, until Ambassador Jon David Glassman in 1991, who the authors argue used U. S. pressure to save democracy in Paraguay.

RENÉ D. HARDER HORST
Appalachian State University

RENÉ D. HARDER HORST. *The Stroessner Regime and Indigenous Resistance in Paraguay*. Gainesville: University of Florida Press. 2007. Pp. 224. $59.95.

This impressive study explores some interesting scholarly territory in a focused and synthetic way. On the surface, René D. Harder Horst examines how the authoritarian political culture of Paraguay treated the most underprivileged class in the country and how those native peoples fought back. But the tale is even more intriguing. While scholars have certainly given indigenous groups in South America a great deal of attention, only rarely does their chosen chronology permit some useful conclusions about the recent past or offer thoughts on current policy. Also, while historians have finally started to examine the dictatorship of Alfredo Stroessner (1954–1989) in detail, they have mostly limited themselves to spotty and rather stale biographical accounts or analyses of foreign relations. Horst, by contrast, tries to see the dictatorship through Indian eyes, and ends up making a decidedly nuanced contribution.

His is a most edifying, if tragic, story. Paraguay is basically a mestizo nation in which Spanish shares the linguistic base on a near-equal footing with the Guaraní Indian tongue. Examples of true bilingualism are rare anywhere on the planet, and, given the Paraguayan government's oft-stated pride in the country's native past, one might think that indigenous people would enjoy a place of honor in today's world. Not so. The historical relation between the Indians and greater Paraguayan society has been fractious and bloody since the time of first contact in the 1500s, and throughout the colonial and early national period, it improved little if at all. Certain areas of the country, most notably the Gran Chaco wasteland in the west, served as perennial battlegrounds between the two groups. These were lands that the Jesuit writer Martin Dobrizhoffer described "as a theater of misery for the Spaniards and for the Savages, as their Palestine and Elysium." Despite the ongoing conflicts, however, real change proved elusive. The stark physical character of the Chaco and the forested areas in the east of the country imposed a kind of historical stasis, wherein a final reconciliation between Indians and Paraguayans could be postponed indefinitely.

Thus, while the struggle between Indians and Europeans had long since terminated in places like Mexico, Argentina, and the United States (sometimes through accommodation, more often through slaughter), in Paraguay events only moved to that stage in the mid-twentieth century. Horst points out several factors that pushed the process along: the migration of outsiders, especially Mennonites, into traditionally Indian lands; the 1932–1935 Chaco War between Paraguay and Bolivia; and modernization of the ranching, yerba mate, and timber industries. Under such pressures, some of which were unintended, the old dichotomous relation with the Indians could no longer stand unaffected. But what would replace it?

The moment for change occurred during the early 1950s, just at the time when General Stroessner came to power in Asunción. People commonly forget how very young and ambitious the general was in those days, and in propelling his government toward his vision of the future, he proved willing to use both modern and heavy-handed means. Although not wholly absent from his plans, improving relations with the Indians was decidedly a low priority in his administration. He entrusted the matter to a newly created Departamento de Asuntos Indígenas (DAI), laying out some broad goals. Stroessner initially favored a paternalistic approach that sought to settle the remaining native population into colonies where they could be protected from cultural extinction. The DAI encouraged the activities of religious missionaries as the most efficient way to handle the experiment, and funneled material aid to the Indians through Protestant and Catholic organizations. As Horst points out, these early efforts failed because no one ever bothered to consult the Indians themselves and because too much graft seeped into DAI operations from the beginning. Thus, the economic plight of the Indians did not improve; it got worse.

In the period from 1967 to 1976, Stroessner abandoned his earlier approach to the Indian question and adopted an open policy of forceful integration. The new attitude, which was diametrically opposed to the earlier paternalism, held that closed communities perpetuated a cultural distinctiveness that was undesirable in a modern, capitalist Paraguay. Aligning himself with the state's position, the head of the DAI, Colonel Tristán Infanzón, never hesitated to destroy native settlements if they "stood in the way of development." He also routinely aided private ranchers—members of Stroessner's own Colorado Party—in breaking up Indian communities and, in effect, selling the survivors into slavery.

The ruthlessness of the campaign against the Indians drew international condemnation and spurred the first inklings of activism among the Indians themselves. Those who sought fundamental political change in Paraguay now had another issue that could help focus their opposition to the dictatorship. And native peoples learned how to mobilize in defense of their lands and rights. They put aside their prejudices against each other and worked hard to adopt a common program.

Horst throws a concentrated light on this process, supplying details on organization and personalities, and pointing up many ironies. He notes, for instance, that

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native leaders collectively demanded respect for their communities and equal rights under the law, but in so doing, they necessarily turned to national political venues. The protests that Indians directed against the dictatorship's social policies therefore enhanced the integration of those same Indians into a greater Paraguayan dynamic. In order to save themselves, they essentially reinvented themselves. This had costs as well as benefits.

Stroessner was overthrown in 1989, but many of the challenges that Indians face in a democratic Paraguay are still daunting. To understand where they came from, and where they hope to go, start with this book.

THOMAS WHIGHAM
University of Georgia

PILAR GONZÁLEZ BERNALDO DE QUIRÓS. *Civility and Politics in the Origins of the Argentine Nation: Sociabilities in Buenos Aires, 1829–1862.* Translated by DANIEL PHILIP TUNNARD. (UCLA Latin American Studies, number 88.) Los Angeles: UCLA Latin American Center Publications. 2006. Pp. xiv, 399. $29.95.

This book, the English translation of the 1999 French original, focuses on the evolution of practices of sociability in the city of Buenos Aires from the mid-1820s to the early 1860s. Following Maurice Agulhon's works, Pilar González Bernaldo de Quirós tries to relate changes in sociability to transformations in the political sphere. This innovative approach permits her to establish connections among Masonic lodges, literary salons, electoral clubs, and other voluntary organizations and the emergence of a republican politics predicated on notions of civility, equality, and contractual association. A series of complex, historically grounded arguments are articulated by a dominant proposition. Certain practices of elite sociability (newspapers, civic associations, and face-to-face interaction at cafés, pubic libraries, and reading clubs) contributed to delimit a new public sphere in which it was possible to imagine the nation as a contractual social bond uniting people with similar ideas about freedom, representative government, and civilized behavior.

The first sign of change was the displacement of *pulperías* (taverns), the meeting place of the working poor, by cafés (coffee houses), places where young men of high culture met and socialized. This generated the first break with "neighborhood sociability" in post-independent society. In the mid-1820s, the Rivadavia administration created the conditions for a bourgeois civility, expressed in benevolent societies, reading clubs, and cafés. By 1830–1838 a clear polarization occurred: on one side were African societies supporting the dictatorship of Juan Manuel de Rosas; on the other were elite associations that opposed it. Around the University of Buenos Aires a "cultivated sociability" formed in literary salons, reading clubs, and study groups. Marcos Sastre's literary salon was transformed from a place for informal literary discussion into a patriotic society with a program of nation-building. During the Rosas dictatorship (1835–1852), civility and politics drew apart. After 1839–1840, intellectuals were forced into exile and, consequently, cultivated sociability dramatically declined. With the proscription of the *unitario* opposition, the number of newspapers fell significantly, and so did the number of free associations.

Conversely, these two key indicators of upper-class sociability rose again after the fall of Rosas and the return of the liberal elite to power. During 1852–1862 a new passion for recreational activities and for associations pervaded the city's elite. *Ateneos libertarios* (working-class literary clubs), scientific societies, and leisure clubs multiplied. Typical were the newly founded Progress Club and the nation-wide reorganization of Masonic lodges. New electoral clubs helped the elite build consensus on lists of candidates for congressional elections. Initially tied to neighborhoods, these electoral clubs soon expanded to form the basis of a peaceful political coexistence among elites. In fact, it was the 1857 gesture of the dominant Masonic lodge—which appointed key political figures to the top grade, without passing through the initiation rites—that created the conditions for the reunification of Buenos Aires State with the rest of the provinces.

This engaging and well-written book makes an important contribution to the history of Argentina. It presents a wealth of information about associations, coffee houses, reading clubs, Masonic lodges, and other forms of elite sociability. The collection of biographic data on politicians, publicists, bureaucrats, members of associations, and university students is truly remarkable. The book achieves the important objective of displacing the analysis of politics to the sphere of sociability. In an erudite and elegant style, the author presents important events in post-independent Argentina in a new light. Behind party politics were electoral clubs; well-known politicians forged social connections at sociocultural associations and Masonic lodges; and new ideas about civility and government found resonance at these types of societies before they reached the press.

Readers with a sensibility for postcolonial and subaltern criticism may find the elitism that this book reflects discomforting. African societies are considered premodern because of their ethnic character, and the newspapers printed by Rosista supporters are dismissed as part of a public sphere too close to government. The only legitimate public sphere is a contractual one, based on liberal-republican principles and supported by practices of sociability by the wealthy. Imagining the nation as a contractual social bond among Argentines could only come from the associations and cultural practices of the *porteño* elite. Everything else was not public opinion; popular expressions not informed by elite civility appear as "infra-political." This is debatable. Since the post-independence period there were other, competing perspectives on modernity, civility, republican government, and nation that are not given equal value in this book. By framing its argument in a too rigid and elitist definition of political modernity, the author has replicated in a new form a tradi-

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tional, elitist vision of Argentine historiography, one that has enshrined the "Generation of 1837" as the true designers of the nation.

The English version's bibliography has not been updated to account for post-1990 publications on the Rivadavia, Rosas, and Organization periods. Due to an unconventional way of presenting the data, the figures are sometimes difficult to read, particularly the "geomaps" representing the spread of taverns and coffee houses, and the bar charts showing the evolution of associative activity. And the translation itself, while generally good, does not entirely avoid misleading (and on occasion, mistaken) words and sentences.

RICARDO D. SALVATORE
Universidad Torcuato Di Tella

JULIA RODRIGUEZ. *Civilizing Argentina: Science, Medicine, and the Modern State*. Chapel Hill: University of North Carolina Press. 2006. Pp. xii, 306. Cloth $59.95, paper $24.95.

This is another book focused on finding what went wrong in the modern Argentine experience. It is part of a sort of subgenre mostly cultivated by U. S.-based scholars interested in offering conclusive explanations of a national performance that was very promising at the end of the nineteenth century—the so-called "golden era"—but transmuted into repression and authoritarianism for most of the last seven decades of the twentieth century.

Julia Rodriguez argues that the "golden era" was not the starting point of a process that later went wrong but the period when the basis of the subsequent authoritarian rule took shape (pp. 3, 250). Rodriguez discusses the role played by certain professionals well established in emerging state agencies, particularly their discourses aimed at the construction of a modern social order. She pays special attention not only to the unexpected and undesired social and political dimensions of late nineteenth-century material progress but also to the sophisticated instruments the modern state developed in order to enforce its agenda of social control, racism, restrictions of individual liberties, and sexism. In other words, this book emphasizes that the turn-of-the-century liberal decades were much more repressive than some historical narratives have tended to suggest.

The book has four main sections. The first, "Symptoms," deals with the formulation of ideas of progress as well as the formation of a scientific/professional elite with very close ties with the liberal state. The second part, "Diagnosis," focuses on the how ideas of abnormality and degeneration were instrumental to a broader model of social control. The third part, "Prescriptions," discusses institutions of regeneration for men and women such as the home, prisons, hospitals, and orphanages, as well as the role of the liberal state—and also the Catholic Church—in efforts to purge the nation of undesirable, unhealthy, and politically dangerous individuals. The fourth part, "Hygiene," focuses on elite discourse about how to select and integrate immigrants in the national "body."

This book contributes to a historiography that from the early 1980s onward identified social control as a historical problem and transformed it into a well-established topic. Rodriguez's work shares both the achievements and shortcomings of that approach. As for the achievements, the most significant one is an emphasis on the regressive dimensions or the liberal project of modernization, bringing in new problems such as the repressive strategies of the modern state and the role of professional/scientific elites in making a new nation. The book's chief shortcoming is its conviction that discourse is the whole story. Without taking into account that discourse does not always become policy, or that formulated policies are not always implemented, Rodriguez seems to assume that rhetoric automatically translated into effective intervention in the public and private spheres and that the new Argentine state was strong enough to put its stated goals into practice without opposition.

In Argentina, as anywhere else, the history of the making of a modern social order has been a complex process wherein reformist approaches embodied sharply and conflicting impulses: authoritarian order as well as social justice, efficiency along with uplift, faith in the power of education and coercion. Hygiene, medicine, criminality, sexuality, and moral issues lent themselves to these contrasting tendencies. Some reformers urged education and others repression. Integrationist approaches coexisted with punishment and segregation, encouragements of healthful amusements occurred simultaneously with movements of censorship.

Rodriguez's book systematically underlines the dark side of these discourses, ignoring those situated in the social reform camp. If things were that negative, however, it is hard to understand why mortality declined, or why literacy levels—albeit not inclusive of the whole society—consistently increased, or why elite social control discourse aimed at selecting immigrants never resulted in quotas or restrictive measures (as it was the case in other countries). Disregarding such facts allows Rodriguez to claim, as she does in her last chapter, that there is a line of continuity between the repressive policies of the "golden era" and the barbarian methods used by the 1970s military dictatorship, when political "disappearances" were recurrent and daily features. The emphasis on continuity, which some readers might interpret as a revisionist periodization of Argentine history, is particularly problematic. Certainly the history of social control illustrates how relevant is the need of for periodization other than that offered by political history. But I doubt this kind of continuity will allow scholars to understand better the modern Argentine experience.

The most convincing discussions in the book deal with discourses focused on criminology, such as the development of classification methods based on physical/psychological measurements of each inmate, supplemented by records of his/her family and hereditary

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history. Also persuasive are the author's subtle reconstructions of the ways traditional moral, sexual, and religious ideas ended up permeating the discourse on modern institutions. The book's subtitle does not reflect this decisive emphasis on criminology. Equally misleading is the geographical scope suggested by the title; in fact, Rodriguez focuses her attention on Buenos Aires, assuming perhaps that repressive discourse reflecting an agenda of social control was evenly represented in a very diverse national geography.

In spite of these shortcomings, Rodriguez has made a valuable contribution to Argentine historiography and to current discussions of the arrival of modernity on the periphery of the industrialized world.

DIEGO ARMUS
Swarthmore College

EUROPE: ANCIENT AND MEDIEVAL

ARTHUR M. ECKSTEIN. *Mediterranean Anarchy, Interstate War, and the Rise of Rome*. (Hellenistic Culture and Society, number 48.) Berkeley and Los Angeles: University of California Press. 2006. Pp. xvii, 370. $49.95.

I hope that Arthur M. Eckstein's book attains the influence it deserves. It offers a sophisticated reading of the ancient evidence in the ongoing debate about the motives or cultural imperatives underlying the expansionism of the Roman Republic, while adding a dose of theory that may make the volume palatable to classicists who have come to consider political history securely grounded in evidence obsolete.

William V. Harris's *War and Imperialism in Republican Rome 327–70 B.C.* resounded like a warhead detonating in 1979. It will probably astonish readers who are not Roman historians to hear that dominant interpretations of Roman history to that date had maintained that the Roman Republic just happened to attain hegemony in Italy and then in the Mediterranean world, without any "policy" or strategic intent, largely because it was afraid of what it might suffer if it did not lash out at other peoples. In the American century such accounts in English came to sound like tendentious recasting of history to justify the American republic's rise to dominance of its continent and then to global power, all in the interests of defending itself, its clients, and global peace. It has not helped that, as others have noted, it is American historians of Rome who continue to "defend" the Roman Republic against charges that it was a pathologically violent culture or an especially rapacious state set on wealth and power for their own sake. I share Eckstein's pessimistic assumption that most classicists and historians of antiquity simply assume that Harris is right to attribute Roman dominance of the Mediterranean world to a combination of a sick cultural proclivity for violence, extreme militarization, and a senate with the morality of a biker gang.

Eckstein's basic point is that "Rome was certainly a harshly militaristic, warlike, aggressive, and expansionist state from a modern perspective, so too were all Rome's competitors, in an environment which was an especially cruel interstate anarchy" (p. 3). In short, Eckstein argues against Roman "exceptionalism" but for "flexibility." Eckstein's conception of the Mediterranean environment of the Roman Republic as a "multipolar anarchy" derives from recent models developed by political scientists, and I share Eckstein's hope that political scientists will read this volume in return and begin a new dialogue, especially as regards realism and idealism in foreign policy. I confess that I find the constant overlay of political science modeling and theory an impediment to getting to the meat of Roman history, yet I am hopeful that the thorough integration of social science terminology will attract classical *litterateurs* who would not be caught dead reading anything so twentieth-century as a sound treatment of Roman political and military history grounded in Theodor Mommsen unless it offered them some new vocabulary.

To summarize: the scale and resilience of Roman success ultimately rested on unique Roman capacity to incorporate others as Romans, what Eckstein calls "the subtle flexibility of Roman identity" (p. 312), not on Roman propensities to be more vicious or greedier or more militaristic than any one else. In addition, centuries-old ideologies of honor, courage, and status drove Mediterranean-wide elites, not just Romans, and militarized whole societies; "empire by invitation" was a very real phenomenon and predated Rome's interventions outside Italy; and (the most old-fashioned conclusion of all) Romans really were afraid of Celts because Celts were genuinely scary. I already believed these things to be true before reading this book, since it is what I see in the evidence. Now Eckstein has done the heroic, painstaking work of generating this 400-page book, saving others the trouble of rethinking the evidence.

This reviewer could quibble with some of Eckstein's lesser conclusions. I think that Greeks did have a somewhat nicer sensibility, if only because they were more embarrassed at butchering people, often feeling the need to explain it. I would locate the genesis of the Roman juggernaut state later than c. 340 B.C., in the early third century. And I might introduce even more factors into the discussion of the utterly stunning decision to launch a war in the Greek peninsula in 200, just a year after the demographic and economic horrors of the Second Punic War against Carthage. (It says something about how successfully Harris recast the terms of the debate that no one other that Eckstein has felt the need to reexamine this "world-historical Roman decision" at such length.) In short, Eckstein does not claim great originality of interpretation in spite of his innovative use of modern social science models. To the contrary, he maintains that Mommsen had it right the first time.

PHYLLIS CULHAM
United States Naval Academy

MIRANDA ALDHOUSE-GREEN. *Boudica Britannia: Rebel, War-Leader and Queen*. London: Pearson Education Limited. 2006. Pp. xvii, 286.

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This is a new book on a topic that never seems to lose its popularity, either in print or on television or film screens. It would seem not an easy task to present the well-known story in a new light, and particularly within the framework of ancient British nationalism, as is the case here.

Miranda Aldhouse-Green has opted to begin her account in the century preceding the Roman conquest of Britain, looking at Romano-British relations from Caesar to Claudius, and attempting to untangle the web of British dynasts, client kings, and rebels only known to us from limited classical sources or from coin inscriptions. She argues that even when native rulers chose for the obverse of their coins images that were Roman in style, the fantastic creatures portrayed were deliberately intended to reflect their difference from Rome. The Iceni in particular seemingly rejected such foreign influences.

Chapters on the Claudian conquest and on client kings follow. On the construction of the Temple to the Divine Claudius at Colchester, the author takes the line that it was begun during Claudius's lifetime and that, as elsewhere in the empire, it was dedicated to the emperor and Rome. On client kings there is a somewhat lengthy discussion of such rulers from many parts on the outskirts of the empire—Gaul, Germany, Judaea, Egypt, Armenia, North Africa—but the position of client king itself is only lightly touched on. The question of possible Roman citizenship for such rulers (and for the three known British client kings) might have been considered here. Not all client rulers were granted citizenship, and it cannot be assumed, as Aldhouse-Green does later in her book (p. 221), that Boudica and her daughters were "free Roman citizens."

Further chapters—on other women of status in Iron Age Europe (known from archacology), on "femmes fatales" (where the author compares Boudica's character with that of her contemporary, Queen Cartimandua of the Brigantes)—continue to set the scene. The prelude to the revolt by Boudica is said to be the assault by Roman Governor Suetonius Paulinus on the Druids on the island of Anglesey, and the Druids seem at least in part to carry the blame for the revolt. The status and power of the Druids were greatly affected by the ongoing influence of the occupying Romans, and there was no place for them in the new social order. The ritual killing of Lindow Man may have been a last-ditch attempt to avert a Roman attack on the sacred island. Native resistance to Roman domination and revival of cultic practices would have been promoted by the Druids, and this could well have included pushing for a united front by the Trinovantes and Iceni.

Finally we come to the revolt, and it is here that the author comes into her element, with good archaeological material to complement the literary sources. In a thoughtful comment Aldhouse-Green interprets the atrocities committed by Boudica's forces on noble Roman women as reprisal for the Roman treatment of the queen and her daughters. For the period following the revolt, the picture is again filled out with up-to-date archaeological evidence relating to Roman retribution and the rebuilding or relocating of the Iceni heartland.

The book ends by comparing Boudica with other historical females, mainly from Britain, and mainly nineteenth and twentieth-century figures. Since it is the author's premise that Boudica should be seen as a "freedom fighter" against Roman oppressors and occupiers of British territory, this might justify including in her group of women the first female Hamas-supporting Palestinian suicide bomber, Reem Saleh Al-Riyashi, who was responsible for the deaths of four Israeli soldiers. One wonders if the portrait would have been as sympathetic if this person had been one of the London bombers.

There are some errors in the book, mainly of historical fact: women were indeed able to conduct businesses if they had the *ius liberorum* (p. 122); gladiators were not all "unfree" (p. 113): some were freed, and others free; use of the *tria nomina* did not necessarily mean a man had citizenship (p. 216); in the classical period women could not have the *tria nomina* because they did not have a *praenomen* (p. 216); a woman could have citizenship because her father was a citizen, not her husband; the burning of Rome is attributed to Nero (p. 112) by Dio and Suetonius, while Tacitus (*Annals* 15.38–39, 44) gives this merely as rumor; and Augustus was not an ancestor of Claudius (pp. 37, 38), merely a predecessor.

But these are small matters, and they are compensated for by the new insights and evidence, and the fresh and modern approach brought to the saga of the British warrior queen. This book is well worth reading.

Dorothy Watts
University of Queensland

Kevin Uhalde. *Expectations of Justice in the Age of Augustine*. Philadelphia: University of Pennsylvania Press. 2007. Pp. 232. $55.00.

At the end of the fourth century, bishops started to preach on daily penance rather than on liturgical penance. Kevin Uhalde puts this important change in the conception and administration of penance into the broader context of the administration of both secular and religious justice. His initial intuition is that "the rise in the person of the bishop as an official who bridged the theory and practice of justice" (p. 8), once Constantine had granted the bishops some judicial role, was a major factor of change both for the conceptions of justice and for expectations about it. Bishops' experience of the failure of justice in this world and their attempt to explain it to their followers changed the way they represented divine justice. But in Uhalde's terms "this is not a book only about bishops" (p. 4). A recurring motif is the ways in which bishops were challenged by the expectations of their followers. Such a focus on the failings of the bishops and on the representations and practices of the laity makes Uhalde's book a new and exciting contribution to the study of late antiquity.

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Chapter one deals with secular justice and focuses on the social problem of calumny. Technically, the Latin term means "false accusation," but in fact it was used to denounce whatever might threaten the administration of justice. Uhalde first traces the concern of emperors with calumny and shows how the multiple layers of legal measures they put in place failed to take care of the problem. He then attempts to measure the importance of the fear of calumny among the people through their use of supernatural "insurance" against injustice, such as oracular texts or judicial curses. Finally shifting attention to the bishops and their courts, Uhalde convincingly shows that bishops share the same concern with calumny: canon law, especially the procedure in the case of clerical accusations, provides a good parallel to Roman law and the measures taken by emperors.

Chapter two is dedicated to bishops. Uhalde sketches the figure of the ideal bishop-judge from the *Didascalia*, where episcopal authority derives from the keys given to Peter, to the Fathers of the Desert, where the central notion is that of discernment. Although these pages are important for the argument of the book, it is quite challenging to go through such an array of sources. Uhalde then attempts to confront the ideal with the actual practice of justice. As expected, cases of failure have rarely been recorded, but Uhalde ingeniously chooses to focus on accusations of sexual misconduct and, from a mass of sources that present only allusions, is able to detail at least one case. A consecrated virgin had been anonymously charged with sexual relations and was subjected by her bishop to a vaginal examination. However, she was cleared of the accusation after she appealed to Ambrose of Milan, whose sister was her close friend. So not only did the virgin's bishop fail to discern the truth (his major mistake being to accept an anonymous accusation), but she was only saved because of her social connections. Uhalde points out that Ambrose left the tension between the ideal and the reality of justice unresolved, but suggests that the next generation, that of Augustine, addressed the issue.

This leads us into chapter three, where Uhalde considers oath-swearing as one of the methods used by the people to control the uncertainties of justice. The New Testament, however, appears to prohibit swearing by the name of God. Augustine, like most exegetes, at first condemned the practice of oath-swearing on this basis. Uhalde convincingly shows that he progressively adopted a more nuanced attitude and that he did so in order to "accommodate social reality." Beyond Augustine, the practice of oath-swearing, which was already part of the life of Christians, took a new religious significance, especially with the involvement of the saints and their relics as guarantors. Uhalde has here a good example of how lay expectations of justice could drive bishops to change their attitude, a better case, I should add, than the one he makes about the consequences of their own failure.

In the fourth and last chapter, Uhalde turns to penance. The bishops tried "to create a community of penitents more inclusive than the living subjects of liturgical penance alone" (p. 133), a community that balances daily sins by daily penance, that seeks mercy and not justice from God. Uhalde draws a convincing parallel between this new representation of penance and experiences of justice. Just as calumny is a permanent threat to justice, one's innermost thoughts are a permanent threat to the ability to earn the rewards of divine justice. People's experience of justice would thus have prepared them for this representation of human fragility. By inviting Christians to be their own judges through introspection, the bishops shared the difficulties inherent in the administration of justice, thus drawing conclusions from their own failure. The parallel is not thematized in our sources, but with it Uhalde very suggestively provides a broader context for the representation of divine justice in late antiquity.

ERIC REBILLARD
Cornell University

PETER SARRIS. *Economy and Society in the Age of Justinian*. New York: Cambridge University Press. 2006. Pp. viii, 258. $80.00.

This excellent book is the first—and much needed—English discussion devoted solely to the economic and social history of the Roman Empire in the reign of Justinian (r. 527–565), a critical period in the transition from the ancient to the medieval worlds throughout the Mediterranean region. Peter Sarris has twin purposes: to establish an economic and social context for Justinian's reign and to consider the period immediately following in the Roman east, before the rise of Islam, in light of his findings. He succeeds in both endeavors.

Sarris argues that Justinian's attempts at provincial reform failed to limit the power of a new imperial aristocracy that had developed in the fourth and fifth centuries. This aristocracy had two aspects. On the one hand, its members linked the world of the imperial court to the world of the provinces from which their revenues derived, but, on the other, these magnates seriously undercut the foundations of the empire by dodging their fiscal responsibilities through large-scale tax evasion, making it most difficult for the government to meet expenses, especially military ones. In the fifth century this had contributed to the collapse of Roman power in the western provinces; in Justinian's age, the entrenchment of the aristocracy in the eastern provinces and the failure of the court to dominate them led directly to Roman failure to halt the Persian invasions of the late sixth century and the Islamic invasions of the seventh. Sarris suggests furthermore that the abuses of the rich contributed to significant resentment and apathy among the peasantry, with disastrous results.

At the heart of this book is the story of the great estates that were the economic anchors of this pre-industrial society. Although the estates existed throughout the Roman world, most evidence for their development comes from papyri that have survived in Egypt. Sarris's painstaking and original analysis of the papyri relating

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to the vast holdings of the Apion clan in Egypt is the foundation of his discussion. He illuminates patterns of labor and administration within the system of great estates along the Nile, and he ventures to make observations about estates in other regions of the empire in the east. Employing as well a vast array of other primary evidence, including numismatic, literary, and archaeological materials, Sarris reveals a complex and fascinating economic world.

Sarris shows that in Egypt the great estates had a bipartite character. Peasants worked partly on allotments of land in return for the payment of rent. They also had to work for wages on centrally managed estate properties that provided most of the marketable surplus from which the estate owners derived their revenues. He argues convincingly that these bipartite estates had developed throughout the provinces of the eastern Roman Empire by the sixth century and lasted in some areas into the eleventh century. Readers will appreciate Sarris's valuable survey of the modern historiography of the great estates, particularly their connections with imperial authorities.

In recent years considerable scholarship about changes in the Mediterranean economy at the end of the classical age has appeared, particularly in English. Readers may wish to consult Jairus Banaji, *Agrarian Change in Late Antiquity: Gold, Labour, and Aristocratic Dominance* (rev. ed., 2007); Michael McCormick, *Origins of the European Economy: Communications and Commerce, A.D. 300–900* (2001); Chris Wickham, *Framing the Early Middle Ages: Europe and the Mediterranean 400–800* (2005); and Bryan Ward-Perkins, "Land, Labour, and Settlement" and "Specialized Production and Exchange" in Volume 14 of *The Cambridge Ancient History*, edited by Averil Cameron, Bryan Ward-Perkins, and Michael Whitby (2000), to name the most important. Sarris's tighter focus allows him to examine the Justinianic age in greater detail than any of these works.

Throughout the book Sarris scrupulously keeps his eye on Rome in the east, an entirely reasonable approach considering his evidence and interest in the economic foundations of the Byzantine world. In doing so, however, he leaves the reader curious about contemporary developments in the western Mediterranean. Readers would value his observations, even in passing, on the great estates in the western provinces recovered by Justinian (Italy, North Africa, and a bit of Spain), and in those lost entirely to Roman rule, particularly Gaul, the region often understood as the original site of "feudal" structures.

This important book deserves a wide audience. Scholars of late antiquity will find it essential reading, as will economic historians of the Mediterranean region, especially medievalists who have an interest in the development of economic structures and their social consequences at the end of antiquity.

MICHAEL MAAS
Rice University

SARAH FOOT. *Monastic Life in Anglo-Saxon England, c. 600–900*. New York: Cambridge University Press. 2006. Pp. xv, 398. $90.00.

For more than twenty years, scholars have investigated the historical meaning and significance of the twin words *monasterium/mynster* as they appear in Anglo-Saxon sources. Sarah Foot has contributed to that investigation in more than a dozen articles and a two-volume study of religious women in Anglo-Saxon England. In this new book, Foot synthesizes for the first time what is known and unknown about minsters between the conversion of the Anglo-Saxons in the seventh century and the first wave of Viking invasions of England, which subsided about 900 C.E. She argues that ardent tenth-century monastic reformers have usually set the tone in modern studies of minsters. Scholars have accepted the reformers' negative judgments on contemporary religious life as more or less true. Bishops Æthelwold (died 984), Dunstan (died 988), Oswald (died 992), and their supporters believed that there had been a golden age of specifically Benedictine monasticism in the time of Bede (died 735). The reformers were sure that, judged against the standard of Benedict's Rule, proper religious life had declined badly and was close to extinction in tenth-century England. The reformers created an enduring image of contemporary minsters as irregular, controlled by laymen, and peopled by negligent clergy who were sullied by abominations. Benedict's Rule had been known and used in the seventh century, sometimes in conjunction with other rules. Foot astutely points out that the imposition of Benedict's Rule as the sole guide for monastic life was a revolution, not a reform.

Putting aside the reformers' condemnations, Foot investigates pre-tenth-century minsters on their own terms. "What was an Anglo-Saxon minster like?" (p. 30) is a simple question with a complex response. To free her readers from the reflex of thinking that all religious houses in the West must have been "Benedictine," Foot generally avoids such loaded terms as monk or nun or monastery. She prefers to write of minsters and religious men and women. Foot argues successfully that minsters were diverse and dissimilar, with a few broadly shared characteristics. She describes a minster as a large or small community, composed of men or women or men and women. The inhabitants of a minster maintained a religious life through collective worship and the observance of some rule or mixture of rules or customs that set them apart from secular communities and aristocratic households. Minsters, which were probably numerous though many were short lived, received gifts from kings and from non-royal landholders, managed estates cultivated by slaves and dependent peasants, had legal privileges, and interacted at many levels with the surrounding world. Some participated in pastoral work, others did not. But beyond such generalizations, there was a wide range in their forms and their functions.

Foot is extremely familiar with the limited number of

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written sources on minsters, which she analyzes both shrewdly and cautiously. There is little likelihood that a previously unknown written source will turn up, but scholars have hoped that archaeology would bring new insight into the history of minsters. In spite of considerable archeological work in recent years, the findings are often disappointing. Although some minster churches were built in stone, the domestic and work-related structures were wooden, as buildings generally were in both ecclesiastical and secular settings. Sometimes archeologists can not be sure if they have excavated a minster or the dwelling of a high-status layman. Written documents, sparse as they are, remain the foundation of Foot's synthesis.

This book is encyclopedic in the sense that many topics are treated systematically with more or less detail. Due to the paucity of Latin and vernacular sources from pre-tenth-century England, Foot can not avoid looking across the channel and the Irish Sea for comparative material. The rules of Benedict, of the Gallic bishop Caesarius of Arles (died 548) and of the Irish monk Columban (died 615), as well as Carolingian documents, sometimes fill gaps in the Anglo-Saxon sources. To many questions she poses, Foot responds wisely that we do not have an answer. Such sensible caution is a strength of the book.

Foot concludes that "we can never wholly comprehend what an early English monastic community was really like" (p. 185). She may have underestimated the insights into pre-tenth-century minsters that her thorough, clearly written synthesis has provided. Her book will be required reading for anyone interested in Anglo-Saxon religious life before the tenth-century Benedictine reforms. One reservation is in order. The archaeological ground plans in chapter three are not interpreted in enough detail. They do not speak for themselves—at least to those of us not intimately familiar with the archaeology of minsters.

Cambridge University Press deserves thanks for producing a handsome volume with a readable type face, good illustrations, and informative maps.

Joseph H. Lynch
Ohio State University

Carole Rawcliffe. *Leprosy in Medieval England*. Rochester, N.Y.: Boydell Press. 2006. Pp. xii, 421. $105.00.

No less than any other type of historical writing, well-conceived and well-written medical history can capture the human condition in its many shapes, shades, and contours. While its theme may be social, technical, or institutional, its resonances are many and varied. Carole Rawcliffe, a seasoned regional and medical historian, has produced a masterful example of such a work. At once it is synthetic and revisionist, building on recent trends in the study of the diseases collectively identified as "leprosy" by premodern cultures while correcting errors and exploding myths. The result is a coherent and well-constructed image of "lepers" and their treatment in medieval English society that is unique and far less bleak than might have been expected. By carefully marshaling her evidence and limiting her scope to medieval England, the author has laid the groundwork for serious comparative studies.

Rawcliffe opens with a discussion of leprosy in the British imperial world of the later nineteenth century, and of the societal conversation it provoked. For religious, medical, and governmental disputants the issues surrounding leprosy's nature and transmissibility were quite significant, as they faced the disease in every corner of the empire. Many fell back on England's (supposed) medieval experience with leprosy, seeing the victims and conceiving of approaches with eyes and imaginations glazed by romanticism, antiquarianism, and anti-Catholic prejudice. For Rawcliffe, however, the issue is not how romanticized medievalism affected British medical policies, but how the use and faulty development of the medieval record resulted in twisted views of medieval reality that still find their ways into historiography (if no longer into policy).

Correction of the record begins with a careful consideration of the ways in which medieval society accounted for the disease's appearance and spread. As with plague and other maladies, there was both a spiritual/theological component and a physiological one. Rawcliffe deftly sorts out the main lines: Old Testament opprobrium and the Christian view of leprosy as "purgatory on earth," a kind of blessing; astrological and behavioral origins (both moral and Galenic); and the vital issue of contagiousness. Medieval English medicine provided no single model and thus society provided no consistent reaction. Shunned mainly for their physical repulsiveness, lepers were sought out by the saintly and supported with alms by the guilt ridden. They were closed away in lazarettos, but these were often located near town gates, crossroads, and bridges for the easy collection of charity. Rawcliffe's treatment of the medieval English leprosarium is both nicely detailed and eye opening. These were not the hellholes that pest houses generally were, but domiciles organized, supported, and run as religious houses, some according to the Rule of Benedict. She explores and explodes the myth of the leper "burial" Mass that welcomed the new resident, replacing it with a far less morbid ceremony of welcome to the community. Many leprosaria charged fees for entrance, and some were so sought after that false lepers had to be turned away and even royal servants could find no shelter. Institutional records of larger houses show that staff members commonly administered palliative therapies as matters of Christian charity.

The book's fifth chapter outlines the medical approaches of the day as dictated by the Galenic tradition. Rawcliffe is generous to medieval medicine and the people who practiced it, emphasizing its holistic internal logic and linkage of method to theory. This internal logic and emphasis on the physical were central to the development of modern medicine, even if they could do little for suffering medieval patients. Together they

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provided a system of inquiry, evidence gathering and analysis, distinction and diagnosis, and logical association of physical treatment with supposed cause that is the foundation of scientific medicine. Medieval doctors could not promise, let alone engineer, a cure for leprosy in the modern sense of eliminating or neutralizing a causal agent, but careful diet, medication, and surgery to alter or remove the most obvious signs of the disease could allow a victim to re-enter society without the social stigma of the "unclean."

From the eleventh to the mid-fourteenth centuries "leprosy" was a label broadly applied to dramatic skin conditions but, Rawcliffe argues, it was not considered especially contagious. Isolation was for care, not social prophylaxis. Scholastic medicine, with its preference for fine distinctions, began narrowing the definition of "leprosy" thereby reducing the number of "lepers," and the Black Death raised the societal (and professional) fears of contagion. And so began the decline of the great English leper houses. As for the eventual disappearance of leprosy from England, Rawcliffe is careful, as usual, to balance factors: the medical redefinition of the disease; improvements in diet and natural resistance; and the rise of immunity-inducing tuberculosis.

JOSEPH P. BYRNE
Belmont University, Nashville, Tennessee

LOUISE J. WILKINSON. *Women in Thirteenth-Century Lincolnshire*. (Studies in History, new series, number 54.) London: Royal Historical Society and Boydell Press. 2007. Pp. xvi, 245. $85.00.

In this work, Louise J. Wilkinson provides a picture of women's activities in an important county at a formative period in the development of both royal and local administration. She does so through various methodologies, including case studies of three noblewomen—Nicholaa de la Haye, Hawise de Quency, and Margaret de Lacy—and prosopographical and statistical analyses of specific groups of gentlewomen, urban women, peasants, criminals, and nuns. Although located mostly in the first half of the thirteenth century, some of her sources—especially those for peasant women—come from the end of the thirteenth or the early fourteenth century. Using a broad range of sources, including chancery and court rolls, bishops' registers, charters and deeds, and even the occasional correspondence, Wilkinson strives to present women's lives in medieval Lincolnshire from the perspective that women's activities were limited by gender-based discrimination on the part of the crown, the church, and society, but that they often transcended those limitations, especially as widows.

This is not a particularly scintillating thesis: indeed, it has become something of a given in American medieval studies especially. What makes Wilkinson's work useful is its broad focus on women from all walks of life. Area studies tend to concentrate not only on a particular locale but also on a particular social class, and works that provide a comparative approach are relatively rare. Wilkinson's choice to examine many different populations of women within one relatively limited geographical area and at one particular point in time leads her to conclude that all women experienced similar kinds of limitations on their activities—they were all disadvantaged in terms of inheritance, public access, and economic opportunities—but that they also all took advantage of "a certain amount of ambiguity" (p. 196) with respect to their status in order to maximize what opportunities were available to them. These are not conclusions to be treated lightly: Wilkinson has incorporated a prodigious amount of archival and primary source material into her study, the product of much diligent effort at both the Lincoln Record Office and the National Archives, and it has paid off in the solidity of her statistical data and the care employed in analyzing them. In the first chapter, she also makes superb use of the Duchy of Lancaster charters relating to her three subjects; this series of records has been underutilized by women's history specialists.

Wilkinson's bibliography of secondary sources is a bit less impressive: it is somewhat dated (very few works after 1999) and heavily skewed toward British historians, with comparatively few works by North American scholars. She mentions in the first chapter that studies of thirteenth-century women are scarce, but she could have used works about women both before and after that century to better effect. This is particularly true for the first chapter, on aristocratic women, in which Wilkinson eschewed the prosopographical approach used in subsequent chapters in favor of presenting the three case studies. Case studies can be quite illuminating, but the author seems to consider them the virtual equivalent of the statistically based presentations in the rest of the book. They are not. This could have been addressed either by analyzing the findings in the case studies in comparison to other works on aristocratic women or by including case studies as examples in the later chapters. The latter course of action would have been much more difficult to accomplish, given the dearth of information available on commoners' lives, but the former could have provided some of the broad sweep that exists in the later chapters and would have made them cohere better. Nevertheless, the whole picture of thirteenth-century Lincolnshire does hold together if the reader develops her own comparative perspective in the process of absorbing that picture's elements.

The book is attractive stylistically and physically. Wilkinson presents her ideas elegantly and without resorting to jargon. The font and layout are clear but not boring. A couple of small errors did escape the vigilance of author and editor, most specifically the characterization of John de Lacy as Ranulph de Blundeville's "son-in-law" (he was Ranulph's nephew by marriage), but on the whole the work was carefully edited and produced.

In sum, this presentation of a slice of medieval English womanhood is both useful, in that it provides a

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great deal of information about the activities of women in thirteenth-century Lincolnshire, and accessible in style. It will not challenge current thinking about medieval women, but it does add nicely to the growing body of work on them.

LINDA E. MITCHELL
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BJÖRN K. U. WEILER. *Henry III of England and the Staufen Empire, 1216–1272.* (Studies in History, new series.) London: Royal Historical Society and Boydell Press. 2006. Pp. xi, 247. $80.00.

Toward the end of this dense but enlightening study, Björn K. U. Weiler states his underlying aim: "to place a ruler's actions in relation not only to the expectations and undertakings of his subjects, but also those of his peers and rivals" (p. 197). He argues that in order to appreciate fully the dilemmas facing him and the actions that he took, Henry III's policies must be analyzed in an "international" context, especially with respect to the Staufen Empire. Weiler argues that a combination of motives underlay Henry's overseas policies: to regain and protect his family's lands in France, to cut a figure among the princes of Europe, and to demonstrate his devotion to crusading. His relations with the empire were essential to achieving any of these aims and entangled him in the politics of Germany, Lombardy, and the papacy.

Weiler organizes his account into three periods characterized by different diplomatic aims. The first was Henry's minority, the second was the beginning of his personal reign from roughly 1235 to 1250 when Henry took up the cross, and the third was the end of the reign. Henry's initial obsession was the recovery of his lands in France, in the wake of the stunning French victory at Bouvines in 1214. Such a goal was not yet unrealistic and was seen as fully reasonable by those around the young king, many of whom, such as Peter des Roches, were French and had served John. Continuity in foreign policy was therefore understandable, which meant that Henry would try to recreate the grand alliance that had failed in 1214 as a means of achieving his goal. Frederick II was the focus of those efforts, for no diplomatic or military maneuvering against the Capetians could succeed without imperial support. This effort, however, brought Henry into the cross currents of imperial politics, involving Frederick's strained relations with the Italian cities, Sicily, and the papacy. Because of his difficulties with the papacy, Frederick could not afford to affront Louis IX. He could only view, therefore, Henry's yearning for the Plantagenet Empire as a distraction and never gave Henry the commitment he hoped for, even after he married Henry's sister Isabel in 1235.

Frederick's excommunication in 1245 and his death in 1250 radically altered political relations in Europe. When he committed himself to a crusade in 1250, Henry acted in the tradition of Christian monarchs throughout Europe, but he also embroiled himself in Mediterranean politics—a logical outcome of which, argues Weiler, was his effort to put his son Edmund on the throne in Sicily. It was an expensive whim and came to naught, although it did have political repercussions in England where it contributed to the launch of the reform movement in 1258. After that, English affairs in Germany were dictated by the election of Henry's brother, Richard of Cornwall, as king of the Romans in 1257. It would seem that at last it would have been possible to use the empire against Louis IX. But Henry was completely occupied with domestic politics and had given up any hope of recovering his inheritance, while Richard was never quite master of his situation and could not have lent much weight against France.

Weiler cites in detail the many diplomatic exchanges between Henry, the empire, and German principalities. Since the actual content of these missions was often concealed, whether intentionally or not, he is forced to speculate about their underlying political motives. His knowledge of affairs in Germany and relations between the empire and the papacy, give his speculation a ring of truth and add depth to our understanding of diplomacy in the period. The thirteenth century was notable for the shifting alliances and loyalties that altered the map of twelfth-century polities. It was also notable for the brooding problem of the Holy Land and the crusades that demanded a concentration of European resources to be successful all of which Weiler clearly outlines.

There are, however, three minor drawbacks to Weiler's account. One is simply the often bewildering diplomatic detail, such that it is often hard to find the forest among the trees. The close attention paid to subtle shifts in policy is worthwhile, but it can be hard to follow. One thing that does emerge clearly is the dominating position of the Capetians in European politics. In a sense, this is really a book about a triangular relationship, for as Weiler clearly demonstrates, in the long run Henry could not achieve any of his overseas aims without either overcoming or cooperating with the French, so that greater attention to France would have been helpful. Finally, Weiler is very generous to Henry. Having established the context for foreign relations, Weiler argues that Henry generally acted rationally given that environment, even in the Sicilian adventure. Indeed, he speculates (pp. 153–154) that the sums demanded by the papacy, though steep, were understandable, and that Henry could have probably come up with the money, which was about as much as was paid for Richard I's ransom in the 1190s. Weiler's speculation prompts an interesting thought experiment: would Henry's subjects have rallied to pay as large a ransom as that paid on behalf of Richard to have him released from foreign captivity had he been imprisoned in the late 1250s?

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CHRISTOPHER TYERMAN. *God's War: A New History of the Crusades*. Cambridge, Mass.: Belknap Press of Harvard University Press. 2006. Pp. xvi, 1023. $35.00.

A specter currently haunts crusade studies, one that Christopher Tyerman attempts to exorcize in this massive book. That specter is Steven Runciman's three-volume *A History of the Crusades*, published between 1951 and 1954 and still in print. Elegantly written and persuasive, Runciman's work continues to shape attitudes about crusades and in fact introduced this reviewer to the subject twenty years ago. One of the most important things Tyerman does differently is widen our perspective of what Holy War encompassed in the Middle Ages and beyond. This is not entirely new, of course; Jonathan Riley-Smith in *The Crusades: A Short History* (1987) chronologically widened the crusading era from 1095 to Napoleon. Thomas F. Madden did the same in *The New Concise History of the Crusades* (2006), extending the era to the battle of Lepanto in 1571, with resonance beyond. Where Tyerman differs from other general accounts is in width and breadth of coverage.

All Western scholars agree that crusades to the Eastern Mediterranean took priority in peoples' hearts and wallets, hence general histories always place crusades to the Levant in top billing. Indeed, Western Europeans obsessed over recovering the land of Christ's passion long after the last major Western outpost fell in 1291. Crusade planners continued to worry more about regaining Jerusalem even when the Turks carved off parts of Eastern Europe in the fifteenth century and got within a whisker of taking Vienna in the sixteenth. But Tyerman explains the crusades as more than simply efforts to conquer and hold on to the Holy Land, even if that remained most important in plans and schemes. He includes substantial accounts of the Spanish *reconquista*, wars against heretics like the Albigensian and Baltic Crusades, and post-1291 military operations to the Middle East. Most of this activity enjoyed papal sponsorship—hence in some form it was crusading—but it differed because much of it was not fought to regain or protect a holy site but rather for other pious and sometimes secular reasons.

Tyerman demolishes Runciman's idea of Western culpability in the death of Byzantine civilization. Runciman partially blamed the end of Byzantine civilization on Latin Christians' general intolerance toward the Greeks and the sack of Constantinople by members of the Fourth Crusade in 1204. Many still believe this latter incident critically weakened the Byzantine Empire and allowed the Turks to bury its all but dead corpse in 1453. This is the chronological equivalent of believing the United States' current overseas woes stem from events occurring during the War of 1812. Tyerman insists that the 1204 takeover of Constantinople was bad but far less savage than, say, the sack of Jerusalem in 1099 during the First Crusade. The Greeks recovered Constantinople in 1261, almost 200 years before its final fall (p. 560). Short of a massive effort dwarfing all earlier crusades, in 1453 there was little the West could do to stop the Turks from taking the city. After it fell the Turks did not force the Greeks to adopt Islam and perhaps most importantly returned Constantinople to the center of a world empire, restoring the equilibrium that had withered away as the Byzantine state declined since the eleventh century (p. 865).

Expeditions to the Eastern Mediterranean may have been the most prestigious and romantic, but Tyerman argues that crusades became most routine in Christian non-heretical areas of Europe such as Italy between the late thirteenth and early fifteenth centuries (p. 894). The reasons are easy enough to see; popes jockeyed for position in a world of Roman factions, Italian discord, and German intervention. To maintain their own independence from ever-encroaching secular powers, popes frequently preached crusades against the enemy *du jour*. In 1378, during the Great Schism, rival popes in Avignon and Italy even preached crusades against each other (p. 900).

Because of the book's length Tyerman can include what other scholars have said in more specialized studies. Its great size, however, is a double-edged sword. Riley-Smith sums up the crusades in fewer than 260 pages of text; Madden in fewer than 230. It takes Tyerman 922. What general reader will take on such a gargantuan account when other serviceable works are a fraction of its length? Tyerman uses most of the standard sources or translations but the endnotes are sparse. His bibliography consists of a five-page "select further reading." While perhaps satisfying the general reader, it requires scholars to page through the notes to find his bibliographical references. Still, as an attempt to cast aside Runciman's well-written but badly dated and often shrill work, Tyerman has succeeded. The small critical apparatus not withstanding, this book should stand as the definitive single-authored one-volume work.

LAURENCE W. MARVIN
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ALAIN BOUREAU. *Satan the Heretic: The Birth of Demonology in the Medieval West*. Translated by TERESA LAVENDER FAGAN. Chicago: University of Chicago Press. 2006. Pp. xi, 255. $30.00.

Alain Boureau's book identifies a theological seismic shift that occurred between 1280 and 1330. During the pontificate of John XXII (1316–1334), in particular, demons would receive unprecedented scrutiny and become a burgeoning theological subfield. At the hands of scholastic theologians, demons would suffer an ethical nosedive, going from relative moral neutrality to unmitigated evil, even as their powers expanded at an alarming pace. The advancing threat of the demon world was complemented by a growing apprehension over the essential integrity of the individual: in particular, concern over susceptibility to alien spiritual influences spiraled. Boureau contends that these changes set the stage for the eventual rise of witchcraft charges. If accepted, Boureau's analysis would displace the cen-

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trality of the Sabbath in current scholarship by establishing the origins of witchcraft some hundred years earlier.

Boureau initially focuses on John XXII's pivotal legal efforts at assimilating magical practices and invocation of demons with heresy (chapters one and two). This revision of a belief-based understanding of heresy to one that accommodated deeds is construed as a reaction against the prevailing tendency to locate sin in the intention and as a complement to the exigencies of current inquisitional protocol. When a series of theologians was called upon to rationalize the assimilation of magic with heresy, the contribution of Franciscan Enrico del Carretto proved the most gratifying. Del Carretto posited that the devil was actually present in the image used in Satanic ritual "as the signified in the sign" (p. 60), hence creating a type of inverted diabolical sacrament. This leads to a nuanced discussion of the Satanic pact, situated within the larger context of the contractual nature of the sacraments and the centrality of the pact in medieval society (chapter three). The relationship between theological inquiry and the expansion of demonic power is then assessed, highlighting the role of inter-mendicant rivalry as stimulant. Thus while Dominican Thomas Aquinas focused on containing demonic capabilities, later Franciscan theologians seemed anxious to effect their release (chapter four).

The remainder of the book focuses on the individual's response to the supernatural, thus plotting an entirely different course with many unpredictable turns. Boureau enlists canonization proceedings to suggest a fourteenth-century assimilation of phenomena as disparate as madness, possession, heretical delusion, and restless revenants (chapter five). This exploration of the fissured human subject is prolonged by an examination of the sleepwalker—"a figure representing the doubling and permeability of the human personality" (p. 149). The phenomenon of somnambulism was apparently non-existent in the early Middle Ages: Boureau associates the emergence of such behavior with the shift to an Aristotelian understanding of the body/soul relation, increasing the body's autonomy with an excess of animal spirits. Already in the thirteenth century the sleepwalker is reputed to attend lascivious nocturnal meetings, hence experiencing a "demonization" that anticipates the Sabbath (chapter six). The final chapter demonstrates how the propensity toward divine and diabolical possession exploits "the zones of emptiness and fragility in the human personality" that have opened up during this period (p. 174).

Boureau characteristically plots an entirely original trajectory, associating seemingly separate phenomena with remarkable originality. There is much to admire: the alignment of John XXII's condemnation of the beatific vision with fear of the fissured human subject is but one dazzling example. Even so, Boureau's chronological framework is occasionally open to question. The claim that there was no particular interest in demons prior to the late thirteenth century downplays the pivotal role of William of Auvergne (p. 94). Moreover, a movement beyond the narrow perimeters of scholasticism to pastoral theologians renders this chronology even more suspect. Writers such as Caesarius of Heisterbach were fascinated by every aspect of demons: their physiology, their psychological angst, their interrelations with humans. Some of the evidence adduced for the fissuring of the individual in this period is likewise strained. For instance, Boureau presents the conviction that each individual is assigned an angel and demon as representing the dualities rife in this period, even though the belief dates back to patristic times (p. 162). By the same token, Boureau credits Olivi with the rediscovery of the view that humankind was created to restore the fallen ranks of the angels (p. 115). But any opinion entertained by figures as prominent as Augustine, Gregory the Great, Hugh of St. Victor, and Aquinas could hardly figure as "a somewhat forgotten or neglected teaching" (p. 115). There is also the question as to why there was the hundred-year hiatus between the invention of this demonological framework necessary to witchcraft and the actual explosion of the phenomenon. This said, there is no doubt that Boureau has written an important book, as much for the questions it raises as for the ones it resolves.

DYAN ELLIOTT
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KATHRYN KERBY-FULTON. *Books under Suspicion: Censorship and Tolerance of Revelatory Writing in Late Medieval England*. Notre Dame, Ind.: University of Notre Dame Press. 2006. Pp. lii, 562. $50.00.

It has widely been assumed that England was largely unaffected by religious radicalism until the advent of John Wycliffe. The heterodox men, women, and movements of the preceding century—Joachim of Fiore, Marguerite de Porete, the Spiritual Franciscans—are commonly presented as European phenomena; even the handful of English scholars whose own work aroused suspicions before the Black Death—William of Ockham, Nicholas Trevet, Thomas Waleys—are thought to have suffered solely in a continental context. It was surely Wycliffe who first fractured the calm of the English academic community, and his early followers who charged the emerging, extraclerical vernacular with the controversial energy so prominent in *Piers Plowman*. And it was in response to Wycliffe that English ecclesiastical and secular authorities first collaborated to create an apparatus for the suppression of texts. Kathryn Kerby-Fulton unsettles these assumptions. She reveals an England *alter*, whose communities, or coteries, of readers were sensitive to currents in contemporary theology a generation before Wycliffe, whose governing elites were well practised as textual policemen, and where (reflecting the decentralized nature of magisterial authority in a realm with few universities but many religious houses and a literate lay elite) a wide spectrum of audience responses was at work beneath the parchment surface, from official cen-

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sorship, via varying degrees of suspicion, to an "intolerable tolerance." This was an England neither contained nor constrained by Wycliffe: the conceptual opportunities for a theological *provocateur* were capacious indeed.

Kerby-Fulton traces the development of these distinctive dynamics of reception in England through readers' responses to revelatory writing—in her view, before Wycliffe, the dominant discourse of radical reform. A timeline of theological turbulence, from the first appearance of the Patarini (Cathari) in 1161, to Reginald Peacock, underlines not only her contention that England had a lively history of heresy but also that its readers and writers had long been exposed to, indeed were deeply engaged with, European heterodoxies. Kerby-Fulton sharpens her focus on the years 1329–1437, which marked, she suggests, the rise of the revelatory genre in England, from the first arrest of an acknowledged revelatory author (the Franciscan, Henry Cossey) to the final record of Margery Kempe's revelations. This was fueled, to a far greater extent than hitherto appreciated, by the texts and tenets of continental theological controversies, of Joachim of Fiore and his Franciscan followers, of arch anti-mendicant William of St. Amour, and of the Free Spirit heresy and its analogues, among which was the*Mirror of Simple Souls*. Already in the first quarter of the fourteenth century, Kerby-Fulton argues, English clerks were alive to the provocative possibilities of these prophetic and visionary voices, as were also the prelates that governed them.

Joachimism, she demonstrates, circulated widely in England, its uneasy passage "a mess of suppression, haphazard survival and secretive hoarding" (p. 74), a manuscript tradition that bears witness both to the interest it aroused and the machinery of censorship it already had inspired. Surviving copies of Joachim's works were handled with a high degree of editorial caution, which extended even to marks of ownership, and the same scruples were applied with the same force to Joachim's followers, Olivi and the Englishman Henry Cossey, condemned for his preaching against John XXII, copies of whose works were consistently anonymized, mutilated, or selectively suppressed. At the same time, the adoption of these strategies attests to communities of clerks in England that were captivated by the prophetic power of Joachim's *Praephatio super Apocalypsim*, or Olivi's *Postilla*. As Kerby-Fulton shows, the provenance of the manuscripts points to a concentration of monastic and mendicant convents and a covert challenge to official theology at the center of the clerical establishment. She proposes the Augustinian friars in general, and the York convent in particular, as a possible powerbase for this project to promote radical prophecy and "to court the laity with ideas of Church reform" (p. 125). In the first of three case studies of *Piers*, she uncovers a Joachimite prophesy contained in a probable York manuscript; she also recalls the connection of the "Bridlington" prophesies to the York friars and the common ground between this text and the case of Friar John, another Augustinian, whose reformist preaching campaign of 1358 contributed to a resurgence of interest in the continent's other corpus of illicit prophetics, the anti-mendicant medley comprising Gerardus's *Evangelium eternum*, the "Protocols of Anagni," and, of course, William of St. Amour's *De periculis*. The old controversy of these texts, and the clamor of contemporary preachers, resounded across the clerical community, and Kerby-Fulton finds its echoes expressed in Latin and vernacular. These were formative influences, she argues, on Geoffrey Chaucer's presentation of the mendicant problem and William Langland's moderation in the A text of *Piers Plowman*.

Nor were these the only visionary voices to penetrate their milieu. The clerical and courtly readerships in which Chaucer and Langland thrived also proved receptive, Kerby-Fulton suggests, to some of the most provocative forms of female revelation, such as the visions of Hildegard of Bingen and the *Mirror of Simple Souls*, and also to some mid-century academic controversies over Ockham and the Beatific Vision. While the watchful authorities added to their armory by applying a form of inquisition, to Friar John, to the would-be beatific visionary, Uthred of Boldon, and even to Margery Kempe, and sought the further restriction of preachers and suppression of texts (e.g., 1389, 1402, 1407–1409), Kerby-Fulton suggests that readers and writers consistently demonstrated a degree of defiance in their continuing engagement with authors and texts publicly acknowledged as dangerous. There was concealed editing at work even in the transmission of Julian of Norwich's *Revelations*. The possibilities of the prophetic genre and the plural responses it provoked in spite of the advance of authoritarian control proved attractive to a cross-section of authors inclined to challenge conventional views.

Kerby-Fulton's two further case studies of *Piers* offer instances in which its early readership—Anglo-Irish clerks, represented by Peters Partriche and Pateshull, the Benedictines of Ramsey—likewise dallied with provocative prophetics that fully realized the propositions only implicit in Langland. The views elaborated by Partriche, Pateshull, and others of their particular milieu, were essentially proto-Wycliffite, and Kerby-Fulton identifies in a number of early Wycliffite works—the *Opus arduum*, Walter Brut—a strong seam of revelatory apocalypticism. Perhaps it was this, she contends, that accounts for theprematurity of Wycliffe's reformation, since the heresiarch himself was so hostile to a tradition that dominated the horizons of his principal audience.

In many ways, this is a bravura display of book history. By building her study of manuscripts as much as texts, Kerby-Fulton has—perhaps fully, for the first time in an English context—uncovered the complex dynamics of learned communities in the Black Death period, the undermighty nature of the Oxford and Cambridge schoolrooms by comparison with the intellectual inventiveness and intrigue, and the publishing power of the communities of mendicants, monks, and profes-

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sional clerks in the provinces. At the same time, she has further eroded the (largely disciplinary) distinction between Latin and vernacular voices of the period: the unbeneficed Langland and the Benedictine Uthred of Boldon belong in the same chapter. She fails fully to convince on the force of official censorship in this period, and the evidence, codicological or documentary, of authors, editors, and readers responding to the realities of repression is more meager than she allows. Inevitably, given the scale of her manuscript survey, which is badly undersold in the unhelpful absence of an annotated list of sources, there are some minor errors. Her prose is somewhat hasty, even breathless as she hurries to cut a new trail through the codicological thicket. Yet there is much here also to admire, and scholars of Latin and vernacular will be grateful to Kerby-Fulton for compelling them to consider the book culture of fourteenth-century England both before, and beyond, Wycliffe.

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Peter Denley. *Commune and Studio in Late Medieval and Renaissance Siena*. (Centro interuniversitario per la storia delle università italiane, number 7.) Bologna: Cooperativa Libraria Universitaria. 2006. Pp. xvi, 495. €45.00.

Peter Denley's book is a richly detailed and archivally sophisticated study of the vicissitudes of the Sienese university (*studio*) from its murky and uncertain origins in the thirteenth century to the founding of the Casa della Sapienza, its most lasting incarnation, in 1416. The author does an excellent job sorting through the sources, both primary and secondary, to give a comprehensive picture of the studio and its relation to the city. His treatment of the archival material is especially noteworthy. It includes examination of a wide range of evidence, from city council legislation to statutes to budgets and much more.

The book is organized thematically into four parts. The first provides an overview of the studio: its personnel, curriculum, physical setting, and sources of income. The revenue included money from the great Hospital of the Scala, a fact that will be no surprise to students of Sienese finance. The second section deals more specifically with the faculty. Denley outlines their terms of service, contracts, salaries, backgrounds (foreigner or native), and involvement in local affairs and politics. The author compares Sienese policy in this regard with that of other cities, and stresses how states competed with each other for the services of the most famous teachers. The third section is devoted to the students. Denley describes student life: their experiences, the freedoms extended to them by city officials, their accommodations, and the availability of books. He attempts to assess the size of the student community and the presence among therein of foreigners, notably Germans and Iberians. The fourth and final section is, by the author's own estimation, the most detailed. It describes in depth the Casa della Sapienza, the state-run student university. Denley shows how it evolved from a derivative institution, whose initial *ordini* were lifted wholesale from its Perugian counterpart, into a uniquely Sienese institution, closely allied to state policy. The book concludes with the conquest of Siena by the Medici and the so-called *statalizzazione* of the Sienese studio, which brought it in line with contemporary Florentine institutions as well as those elsewhere in Italy.

Although Denley effectively conveys details, he is much less successful in dealing with broader meanings and significances. He adduces comparative material but does not integrate it with current themes addressed in the work of other scholars such as Robert Black and Paul F. Grendler. His arguments are often involuted, dealing with small points of distinction and the limits and strengths of archival sources. Overall, the book strikes an uncomfortably defensive tone. Denley argues from the outset for the uniqueness and relevance of Siena and its institutions against those who would claim "decline" and thus consign the commune to a historiographical oblivion (pp. xi–xii). But the author argues mostly against a phantom, which leads him at each turn to stress the special characteristics of the studio, wasting much energy along the way that could have been more profitably employed elsewhere. Uniqueness is not a particularly large claim, and we may easily concede this to the Sienese (or any state). What warrants greater discussion, based on the author's own evidence, is why it took so long for Siena to establish its studio, and why the city seems so often to have lost in the competition with other states. In Denley's apologetic schema, the profusion of laws and legislation issued by Sienese officials for the studio become "relentless experimentation," which he characterizes as a virtue. More generally, Sienese administrative policy and frequent tinkering are seen as "distinct features," which place the city above its peers (p. 3). But frequent legislation and administrative tinkering may also be taken as problematic, as proof, as legal scholars have long pointed out, that local policies were not working well. This conclusion in fact matches well Siena's many failed attempts, dating back to the *dugento*, to establish its university, failures that confuse the reader given the author's optimistic outlook. What is also confusing is the precise meaning the author gives to the term "studio," as the earlier incarnations do not necessarily seem the same as the later one. One wonders, too, about the economic side. Denley notes that the studio's first official charter in 1357 (from Emperor Charles IV) came at a bad time, during war and economic crisis. But much of the author's material suggests that basic to the establishment of the studio was a response to just such times, to attract students and their money and to improve the intellectual capacity of the populace.

It must be said, however, that Denley does not make claims that he does not fulfill. He calls his approach "anatomical," and that is what it is. In this sense he is

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quite successful. A more basic problem here may well be in the manner of presentation. The book reads too much like a thesis. Each section has its own introduction, with significant qualification, followed by subsections with their own qualifications. This interrupts the narrative flow and limits the author's ability to express overarching themes. But Denley has in any case provided scholars a wealth of detail and new material.

WILLIAM CAFERRO
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JARBEL RODRIGUEZ. *Captives and Their Saviors in the Medieval Crown of Aragon*. Washington, D.C.: Catholic University of America Press. 2007. Pp. xxiii, 225. $64.95.

Captivity was a very real danger in the medieval Mediterranean world, especially in eastern Spain, along the frontier zone between Muslim Granada and the Christian Crown of Aragon, in the period from the thirteenth through the fifteenth century. As its title indicates, Jarbel Rodriguez's study examines both the experiences of Christian captives in Muslim lands and the efforts of their families and others to save them from their captivity. Through this dual focus, Rodriguez seeks to place "captives in their proper context within Aragonese society at the center of a complex web of social, economic, religious, and diplomatic networks" (p. xx). The book is divided into two sections, each composed of three chapters. The first concentrates on the captives themselves, looking at how they became captives (chapter one, "Raiding and Piracy"), and their experiences while living in Muslim lands (covered both in chapter two, "Life in Captivity," and chapter three, "Captives and Renegades"). The second half of the book examines the saviors back home in the Crown of Aragon, who worked to ransom their captive relatives and coreligionists. Chapters four and five investigate different spheres of the liberation effort, first at the level of the family and second at the level of the state and other organizations, such as the religious-military orders dedicated to ransoming captives. The final chapter, chapter six, looks at the financial aspects of ransoming captives. The volume concludes with an epilogue considering what happened to captives after their redemption and return to Aragonese society. Throughout, Rodriguez draws on a wide range of sources, including a large number of unpublished royal and notarial documents from the Barcelona archives.

Captivity and enslavement are inherently painful subjects, and Rodriguez discusses them with sensitivity and empathy, especially in vivid descriptions of the wretchedness of Christian prisoners. Yet there is reason to be wary of such a simple response to this topic. Even though Christians in late medieval Aragon may have suffered deeply as they or their loved ones were captured and enslaved in Muslim lands, one must keep in mind that they were themselves capturing and enslaving Muslims in the same period. Thus, while it is perfectly justifiable for Rodriguez to decide to study Christian captives and not their Muslim counterparts (he makes this explicit in his introduction), he cannot ignore the fact that these were two parallel tracks. This was a society that was perfectly comfortable with the idea of captivity and slavery (so long as it was others who were enslaved), which makes it exceptionally difficult for us truly to understand and empathize with the medieval situation. This also makes modern analogies—like the one that opens the book (p. xi), concerning Christian missionaries ransoming captives in the Sudan in 1998—imprecise and misleading. It is hard to see how Rodriguez can argue, from this anecdote, that "medieval history . . . can inform our present condition" (p. xii).

There are moments throughout the book when Rodriguez, apparently following the lead of his medieval Christian sources, almost seems to adopt their world view. There is a certain one-sidedness to a story narrated in chapter six, about a Christian Aragonese man, Fernando, who escaped from captivity in Granada with the help of a local Muslim who wished to convert to Christianity. The pair eventually made their way to lands within the Crown of Aragon, where the Muslim companion was immediately enslaved and sold, despite having converted and changed his name to Johan. Fernando tracked him down and arranged for his freedom with the help of authorities in Valencia, who "worked out an exchange in which Johan's master received another black slave in return for Johan's freedom" (p. 179). Rodriguez concludes the tale without further comment on this irony, apparently seeing it merely as a story of successful liberation. Earlier, after a rather dry description of the process by which Christian families could purchase Muslim captives to exchange as ransom for imprisoned relatives (p. 117), Rodriguez moves on to an emotionally charged account of the sufferings of these Christian prisoners (p. 118). His approach to conversion is similarly weighted. Despite medieval Christian perceptions, conversion to Christianity was not always positive (Johan's tale demonstrates the potential complexity of a convert's status), nor was conversion to Islam necessarily a fate equal to death. Instead, although both Christianity and Islam officially abhorred apostasy, the decision to convert in either direction was highly individual and variable in degree; it could be freely chosen or forced, based on belief or pragmatism, and it was even sometimes reversible. Yet Rodriguez ends his text with the opinion that rescued Aragonese captives were the "fortunate ones—the ones who had been freed and not left to die in captivity or converted to Islam" (p. 192). Perhaps, in an odd way, this presentation does "help us better to understand Muslim-Christian relations in the later Middle Ages"—one of the goals that Rodriguez states at the start of the book (p. xviii)—at least from the Christian point of view.

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EUROPE: EARLY MODERN AND MODERN

ALIX COOPER. *Inventing the Indigenous: Local Knowledge and Natural History in Early Modern Europe*. New York: Cambridge University Press. 2007. Pp. xiii, 218. $75.00.

This excellent book is a useful addition to the recently burgeoning literature on the history of natural history. Its five chapters include one on the compilation of lists of plants growing in various differently defined areas, which includes fascinating material on the early recognition of the importance of what are now called field trips. Another substantial chapter deals with collection of data about rocks and minerals in different regions, and another focuses on the development of those all-encompassing natural histories of different regions which are usually seen as part of a Baconian impulse. Although these three areas have been fully discussed in earlier studies by other scholars, Alix Cooper nevertheless adds a wealth of interesting and significant detail by looking at less well known authors and their works, revealing how the lessons of earlier scholarship can be extended to cover other parts of Europe, and other thinkers. The result is a valuable addition to the literature on the development of early modern natural history.

One of the undeniable strengths of the book is the richly textured and carefully nuanced descriptive history of its subject matter. Although the book also purports to offer an argument about the development of natural history, this is perhaps less convincing. Part of the problem is that the author seems to be trying too hard to convince the reader that an important element in early modern natural history was what she presents as an increasingly higher valuation of the local or the indigenous. In her introduction, for example, where she makes her claims about the "invention" of the indigenous, Cooper presents this supposedly new focus largely as a backlash against the interest in the exotic resulting from overseas exploration—or at least as a new awareness stimulated by discovery of the exotic. There is undoubtedly some truth in everything she says, but it is also true that the world of which she speaks always was a world of the local and indigenous. This is most clear in the material discussed in chapter one, on indigenous medicine. The Hippocratic treatise *Airs, Waters, Places* made it clear that doctors need to focus on local conditions because these are the most significant factors in determining the most common local diseases. Furthermore, it was a commonplace of the medical tradition that local diseases were best treated by local remedies (p. 30).

What was new in the literature Cooper studies was not so much the notion of the indigenous as the rhetoric about the indigenous. Before all the excitement caused by the discovery of New World plants, writers had no need to defend their local studies, and the local focus remained entirely implicit rather than explicit. Accordingly, the backlash that Cooper relates may not have been a real rejection of the exotic but merely part of a rhetorical stratagem for promoting one's own importance as a man with local, and therefore more relevant, knowledge. The invention of the indigenous was largely a rhetorical move, therefore, and yet Cooper for the most part presents it as though it represented a genuine innovation in natural history.

There are also a number of aspects of the story that might have been discussed but were omitted from Cooper's account. Religion is hardly allowed a mention, even though the nature of God's creation and providence were major aspects of thinking about nature throughout the period under discussion. Although the author makes it clear that pocket-sized handlists of local plants were a novel feature of the period, they are seen only as the result of what she presents as a new demand for local knowledge. There is no consideration either of the impact of the printing press, or of the perceived need for new kinds of academic writing after the demise of the almost exclusive domination of the commentary tradition. Then, as now, after all, an academic worth his salt had to write something, and handlists of plants and animals might have seemed an easy way to get oneself into print and to curry favor with potential patrons. Finally, although Cooper's account, in its final chapters, takes us into the eighteenth century, the heyday of classification, she offers no suggestions as to how, or indeed whether, the story she recounts might help us to understand the development of classificatory schemes. Even so, one only notices these sins of omission because the author otherwise presents such a rich account.

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LYNN FESTA. *Sentimental Figures of Empire in Eighteenth-Century Britain and France*. Baltimore: Johns Hopkins University Press. 2006. Pp. viii, 300. $55.00.

Lynn Festa's engaging and erudite book studies the relation of sentimental literature to the burgeoning empires of late eighteenth-century Europe. The book is beautifully written, epigrammatic, and often witty. The author's readings of a wide array of novels, poems, philosophical texts, and iconic abolitionist images and objects are consistently subtle and compelling. Sentimental tropes and figures, Festa argues, performed invaluable, and ambivalent, work in an age of imperial and commercial expansion. Sentimental texts enabled readers to envision the abstract forces such as credit and markets that produced global effects from individual actions; but they also held out the promise that certain realms could be sheltered from the rampant commodification of social relations. Sentimentality helped to produce sympathetic identification with distant others, while also placing comforting limits on the circulation of emotion. It at once allowed readers to grapple with the violations of Enlightenment moral norms that empire so often entailed, and also forestalled certain kinds of critical response to those injustices. The considerable limitations of sentimentality as a critical

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device, Festa suggests, contributed to its popularity. Sentimental readers are prompted to pity, and self-satisfaction at their own sensitivity, rather than solidarity or outraged action. They enter into emotional communion not with the ostensible objects of their sympathy but rather with other like-minded (or like-feeling) souls: the sentimental form fostered unity among abolitionists, not closer community or mutual understanding between abolitionists and slaves.

As Festa argues, sentimentality, in probing the bounds of sympathetic identification, concerns itself, whether explicitly or implicitly, with what it is to be human. Some of the book's most fascinating reflections concern tropes and modes of personification: apostrophe and, especially, prosopopoeia, in which a thing or an abstraction is presented as a person, or in which (writes Festa, following Paul de Man) a speaker, in addressing such a silent or absent other, invites it to respond and so confers on it a human voice. Tales told by things—coins, pocket watches, snuffboxes—enjoyed a vogue during this period, as such personifications gave shape to the inconceivably vast operations of global commerce. Those tales, in which objects narrate their dizzying circulation among and misuse by various owners, and yearn to be possessed by someone of virtue and feeling, gave voice to the era's disquiet about commerce, which threatened to make everything fungible, nothing and no one distinctive. The narratives anxiously glimpse a world of Marxian commodity fetishism, in which people can perceive their relations with one another only as associations among things. Festa shows wonderfully that these texts seek precisely to forestall such an existence by insisting, for all their fascination with exchange, on the sentimental rather than commercial value of particular objects. If participants at the time were surprised when emotions themselves became commoditized, as when a poignant moment in Laurence Sterne's *Sentimental Journey through France and Italy* (1768) sparked a mania in Germany for horn snuffboxes, Festa demonstrates that such traffic in passions was precisely to be expected from the sentimental mode. She explores the "harrowing proximity" of slave narratives to the object tales. This was a discomfiting similarity that the slave narratives themselves played upon, as when Olaudah Equiano recalled his initial terror at the apparent animation of European clocks and painted portraits, or when he and many others recounted their efforts to converse with the Bible after seeing Europeans moving their lips as they held the book in prayer. These category mistakes, as Festa notes, wryly expose the willful mutation of subjects into objects, human beings into things, that was chattel slavery.

In a characteristically acute phrase, Festa notes that sentimental works engaged in the "redundant humanizing of the already human" when they sought to "confer a human face and form upon the victims of empire" (p. 8), and her account of the ideologically ambiguous work of the sentimental is one of this book's many great contributions. Some contemporaries—she mentions Mary Wollstonecraft, Samuel Taylor Colderidge, Anna Letitia Barbaud, and William Cowper—recognized the moral and political weaknesses of the sentimental mode and subjected the form to scrutiny, criticizing its tendencies to allow narcissism and self-congratulation to substitute for action. Despite such moments, one might worry—and this is indeed a quibble about a remarkably subtle, powerful, and wide-ranging book—that Festa does not fully acknowledge the varying degrees of self-consciousness and critical-mindedness with which sentimental tropes and ideas were deployed. Citing Adam Smith's famous account of the "man of humanity in Europe" who can sleep soundly after hearing of a catastrophic earthquake in China, but not after a minor injury to himself, for instance, Festa comments, "Smith's little finger dwarfs the death of millions" (p. 213). But Smith's point in the passage is precisely to note the inability of feelings alone, without reflection and communication, to produce adequate moral judgments. Smith, Denis Diderot, and others were acutely aware of both the powerful moral work that sympathy can perform and its limitations and excesses. The fact that many others deployed sentimental figures in self-indulgent or self-deluding ways should not, as Festa herself notes in the book's thoughtful closing pages, be an indictment of sympathy and sentiment *tout court*, for they may, with all their dangers, be indispensable for the arduous and still urgent task of imagining others without presumption.

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EILEEN HUNT BOTTING. *Family Feuds: Wollstonecraft, Burke, and Rousseau on the Transformation of the Family*. Albany: State University of New York Press. 2006. Pp. xiii, 257. $65.00.

Eileen Hunt Botting's book is the first to provide a sustained comparative focus on how Jean-Jacques Rousseau, Edmund Burke, and Mary Wollstonecraft conceptualize the family, how these conceptualizations undergird their political philosophies, and how exploring this interaction reveals surprising similarities among three writers more frequently construed as "archenemies" (p. 9). Botting's recharacterization of them as "sibling rival[s]" rests on three claims (p. 191). All three authored major philosophical responses to the emerging egalitarian family. All three viewed the family as the primary venue for inculcating the moral affections on which civic life in its various forms relies. All three identified the family as the chief corrupting or regenerative basis of the state. Botting grants that these shared tenets were used by each to justify very different political ends and that both Rousseau and Burke opposed any weakening of the family's patriarchal structure. Still, Botting asserts not only that we gain a clearer conception of the aims and consistency of each of their political writings by attending to their shared endorsement of the "little platoon" model of family life but also that their specification of family as the schoolhouse for

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moral and civic virtues is "one of the most important, yet largely ignored, legacies of Enlightenment political thought for the modern liberal tradition" (p. 210).

These twin convictions shape Botting's chapter-length analyses of the major works of Rousseau (chapter one), Burke (chapters two and three), and Wollstonecraft (chapter four). Commendable are the range of writings engaged, lack of discrimination between non-fictional and fictional works, and welcome restraint in not discrediting any of their views on family by delighting in his or her flawed performances as family members. Admirable, too, is Botting's grounding in major literary-critical and feminist accounts of these authors. Each chapter shows how sustained focus on family recasts standard conceptions of each writer's methods and claims. Attending to Rousseau's delineation of the rural family resolves the apparent tension between his views on popular sovereignty and the retreat of his model family while demonstrating how his novels and political works function as interlocking pieces of a philosophical puzzle, each one picking up where its predecessor left off. Examining Burke's writings enables a benevolent view of his obsession with preserving the hierarchical family, less as the sphere of male privilege than as the only transcultural grounds for a nation's "sociability and stability," an assertion that is grounded philosophically in the affective-aesthetics of the beautiful (p. 73). Though more expressly egalitarian, Wollstonecraft's family, too, is promoted as the affective space in which future citizens are formed and thus reveals the "morally conservative, Romantic, and Christian theological strands" of her thinking (p. 133).

I agree that each of these writers' political philosophy is centrally informed by their thoughts on the family and that understanding this interaction is a crucial bequest to modern liberalism, but the lessons I would draw from this are less sanguine. What their writings reveal is a major benefit of anchoring one's political theories in discourse on the family: claiming implicit moral authority for one's deliberations on society—something these writers all need, owing to the wildly paradoxical nature of each of their bodies of writing. Moreover, comparative focus on their writings underscores the power of rhetoric in political argumentation, for each champions the same terms ("family," "natural," "affection," "reform") but means very different things by them. Surely it matters that only Wollstonecraft's family is forward looking; that its value requires, rather than resists, a revolution in female manners; and that marriage is ratified to the extent that it is indistinguishable from friendship, by which she means a relation based in reason and equality. Nor does her family endorse the psychology of Burke's little platoon or Rousseau's "small fatherland" (p. 24), which elicits affection by cultivating an us-against-them mindset, whereas Wollstonecraft's, like William Godwin's, aims to extend the sphere of relations beyond blood. No writer is better than Burke at demonstrating how to influence—i.e., silence—public debate: simply declare that "it is natural to feel this way." Plus, there is nothing more natural than claiming that family is the sphere of affection, despite a long history, beginning with Rousseau, of defining it as the first social grouping one enters upon leaving the state of nature and thus one's first foray into the sphere of aggression, competition, and rivalry (pp. 33–36).

Put another way, Botting's eagerness to characterize these three powerfully intelligent, rhetorical, and prescient writers on the family *as* family may flatten their internal complexities as well as the many differences among them, but it accurately depicts what the platoon-like family fosters: feeling justified in disliking those who think differently from "us."

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Stefan Goebel. *The Great War and Medieval Memory: War, Remembrance and Medievalism in Britain and Germany, 1914–1940.* (Studies in the Social and Cultural History of Modern Warfare, number 25.) New York: Cambridge University Press. 2007. Pp. xviii, 357. $90.00.

Because they survive in the tens of thousands, war memorials resist the kinds of generalizations that cultural historians prize. Stefan Goebel narrows the field to the commemorative use of medieval themes. He positions his "empirical comparative history" (p. 6) between views of postwar commemoration as personal mourning and views of it as political mobilization (p. 2), eventually arguing for the validity of both perspectives. Medievalism, English or German, gets only a cursory exposition; Goebel calls it an "amalgam of temporal notions" rather than a "coherent intellectual position" (pp. 10–14). This book itself, unfortunately, might be described as an "amalgam of contemporary notions," organized around indistinct and overlapping themes.

The first chapter, "Catastrophe and Continuity," shows how mourners contextualized the war in terms of classical as well as medieval narratives. Chapter two, "Mission and Defence," concerns crusade rhetoric and distinguishes between "dominant narratives about 'the war' in general" and "sub-narratives" specific to various theaters of war (p. 154). Crusader themes were more popular in England than in Germany but were "very notable in the commemoration of individuals" in both nations (p. 123). Chapter three ("Destruction and Endurance"), shows how iron, steel, stone, rocks, and ruins were used to represent the experience of the war in memorials; Goebel juxtaposes the German "Iron knight" to the English "gentlemanly knight" (p. 186). Chapter four ("Chivalry and Cruelty") examines the representation of what the author calls "the soldiers' murderous deeds" (p. 27) in the iconography of chivalry. One section is quirkily entitled "Knights and Butchers" (pp. 189–216); the few pages therein on atrocities (pp. 207–216) conclude that such events "had no bearing on signifying practices" (p. 216). Unless Goebel considers "butchery" and atrocity to be norms rather than exceptions (a distinct possibility, it seems),

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the relevance of this material to the argument is not obvious. The last chapter, "Regeneration and Salvation," ostensibly argues that medievalism tried to give "transcendental meaning to these human losses" (p. 27), although the conclusion claims that memorials sought "redemption through tradition" (p. 286). Which is it? Art positioned between the medieval past and modern continuity historicizes experience rather than transcends it; such art is time-bound, not timeless.

Goebel's separation of general war narratives from narratives specific to certain theaters is a rare moment of organizational clarity. For the rest, he would have done better to categorize memorials in the order in which they were dedicated; compare those in urban centers to those in villages; and otherwise pursue clear and helpful structures. Undisciplined language is a greater weakness. Goebel's reference to "the soldiers' murderous deeds" (p. 27), for example, fails to distinguish lawful from unlawful killing. The war saw plenty of both kinds; Goebel's reason for ignoring this crucial moral difference is unclear, unless he thinks that all killing in war is wrong. He writes that "the war dead were not only victims but also executioners"—that is, those who put others to death (p. 200). But this is untrue: not every soldier who died in the war killed someone else. Goebel describes chivalry as "an essentially classless rhetoric about national virtues" and a language that "elevated the common soldier and thus diminished the gulf of understanding between classes." Such aspirations, he argues, aimed to "dissolve" the class dichotomy "typical" of the nineteenth-century medieval revival (p. 200). However, chivalric memorials in some English parish churches were donated by prominent families in memory of their own sons, while similar memorials in other churches honor all the sons of the parish. The former examples, hardly rare, disprove Goebel's claims for dissolving class dichotomies; elsewhere Goebel comments on the pluralism of memorial culture and the "existing cleavages in society" it underscored (p. 289). He cannot have it both ways.

The book is replete with similar contradictions, empirical and otherwise. Goebel refers to the "unwritten laws of chivalry" (p. 199) without explaining how he knows about them (word of mouth?) or why he ignores the written ones that were fundamental to nineteenth-century medievalism. The Passion of Christ, he writes, was "endlessly recalled in war memorials" (p. 233). But on the same page he comments that fear of Catholic associations limited the role of the cross—and of course the crucifix—in British memorial art (p. 254). If the crucifix could not be used as a popular icon in war memorials, how could recollections of the Passion be "endless"? Many crosses, as he observes, were designed so as not to look too Catholic. Goebel asserts that the "romanticism" of earlier memorials seemed inappropriate after World War II, although, as he notes, the names of the newly dead were often added to those same memorials (pp. 291–292). Perhaps they were not as unsuitable as Goebel would have us believe. Not all chivalric memorials are "romantic," in any case; not every image of a knight conceals the price of victory or, by allusion, implies that warfare is bloodless or glorious. References to "slaughter," "butchery," and the "sanctified status of soldiers as killers" (p. 231) are pointedly derogatory; since it is inaccurate, this language is also unproductive. The index, important in even a well-planned study, is spotty and, for key entries (chivalry, the Passion), inadequate. Readers who can shape their own patterns in the evidence and ignore injudicious language will find things of value here, but a dispassionate and systematic book would have been more useful and much more persuasive.

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Jo FOX. *Film Propaganda in Britain and Nazi Germany: World War II Cinema*. New York: Berg. 2007. Pp. x, 358. Cloth $94.95, paper $29.95.

Making propaganda films is challenging. People attend to the daily news, even if it has heavy propaganda content, from the understandable desire to know what is going on in the world, but seeing a film is usually voluntary, and both the British and Germans disliked paying good money for bad propaganda. Jo Fox's study compares how Germany and Britain attempted to put their respective propaganda into films that attracted audiences. It is well grounded both in archival and secondary sources and provides good discussions of individual films, but the overall analysis is sometimes unsatisfying.

There are many books on British and Nazi cinema during World War II. Comparative studies are less common. Michael Balfour's idiosyncratic but lively *Propaganda in War 1939–1945* (1979) compared British and Nazi propaganda, without giving major attention to film. Richard Taylor's *Film Propaganda in Soviet Russia and Nazi Germany* (2d ed., 1998) compared two dictatorships. Fox's book is the first, in her words, to "analyze historically the relationship between British and German cinema of World War II and compare and contrast their propaganda methods in the tradition of attempting to locate film within its political and historical *milieu*" (p. 6). This passage also illustrates the writing style, which is clear (a blessing, given the obtuseness of much film scholarship) but could be more engaging.

After an introduction that summarizes the conundrums and controversies film scholars face and outlines the ground to be covered, chapter two compares the opening films of the war. The Germans had the easier job of it, both in material and finances. Fox focuses on two films used to justify the war, *Feuertaufe* (April 1940) and *The Lion Has Wings* (November 1939). The British refused to show *Feuertaufe*, fearing its impact, but Joseph Goebbels thought the British film so bad that he considered releasing it for German audiences. Chapter three, the strongest in the book, compares films on the air war, which in Germany involved heroic and idealized pilots who often died, while Britain favored brave,

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spirited individuals standing firm under German bombs. Chapter four considers the portrayal of the enemy in films such as *Ohm Krüger* (1941) and *49th Parallel* (1943). In general, both systems targeted the enemy's leadership rather than its citizens. Chapter five reviews the familiar ground of historical films. Germany found analogs for Adolf Hitler in Frederick the Great and Otto von Bismarck; Britain saw Winston Churchill's predecessors as William Pitt the Younger and Benjamin Disraeli. By chapter six, the trajectory of the war has changed, with the British producing films suggesting that victory would overcome past social problems, while the Germans focused on escapist films like *Münchhausen* (1943) and injunctions to fight to the death as in *Koberg* (1945).

The conclusion could be stronger. The point of the exercise is to compare how propaganda functioned in two different societies. Although earlier chapters make clear and interesting comparisons that are summarized in the conclusion, the result does not entirely meet the promise of the introduction. For example, Fox begins the chapter by citing John Grierson, who saw propaganda as an urgent necessity for democratic states, then cites Jacques Ellul's claim that democratic propaganda is "ineffective, paralysed, mediocre" (p. 314). Ellul's point is not that democracies cannot make effective propaganda, but rather that doing so requires a surrender of democratic principles. It would be worthwhile to think more about whether the increasing effectiveness of British film propaganda reinforced or weakened democratic convictions.

There are minor errors and confusions. A December 1944 report that Germans were boycotting newsreels is used twice (on consecutive pages) to support claims that they were tired of newsreels in March 1943 and March 1942 (pp. 257–258). A reluctance to see newsreels is understandable late in 1944, when the news was uniformly bad, but the evidence does not support the claim that interest had declined when there was still good news. Fox is sometimes confused about details of the Nazi propaganda system, referring, for example, to a "Reichspropagandaleiteramt" (p. 267), which confounds the party and state propaganda hierarchies. Each Gauhad a Reichspropagandaamt subordinate to Goebbels as Reich Minister, and a Gaupropagandaleitung subordinate to him as head of the party propaganda system. The same person headed both offices. There are other such problems that do not vitiate the overall analysis, but do annoy.

In sum, this is not the best book either on Nazi or British war-era cinema, but its consideration of both makes it distinctive and useful.

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Barry Eichengreen. *The European Economy since 1945: Coordinated Capitalism and Beyond*. (The Princeton Economic History of the Western World.) Princeton, N.J.: Princeton University Press. 2007. Pp. xx, 495. $35.00.

Economic experience in Europe from 1945 to 1989 can be divided into four unequal parts, separating Europe geographically into East and West and temporally into periods of strong growth to the early 1970s and slow growth since 1973. Since 1989, the transformations of Eastern European economies and the renewed integration effort have created a European economy with great potential and with significant difficulties to be overcome. Rapid postwar growth in the West has received far more attention from economic historians than later developments. Barry Eichengreen's history of the European economy since 1945 follows this pattern, devoting more attention to the West and the era of strong growth, but it provides a valuable overview that includes two chapters on Eastern Europe, substantial attention to the slower growth era since 1973, and two chapters on the challenges of converting Eastern bloc countries to market economies and the furthering of continent-wide integration since 1989.

This book sets a new standard for surveys of the period, outclassing the essay collections that have concentrated on Western European experience and single-author narratives that have tended to make dreary reading. Eichengreen has produced an invigorating blend of synthesis and analysis that poses major questions about the nature and evolution of European economic growth, surveys economic arguments, and delivers sharp analysis and clear explanation for the major phases of economic growth and integration. It should be essential reading for historians of modern Europe wishing to understand European experience since 1945, and it is an indispensable complement to Tony Judt's superb *Postwar: A History of Europe since 1945* (2005), which gives little attention to economic development.

Eichengreen's account of the "golden age" of European growth from 1950 to 1973 emphasizes the growth made possible by catch-up and convergence: recovering from the slower growth during the Depression and World War II, and narrowing the productivity gap between Europe and the United States by adopting American technology and methods for mass production. He carefully assesses contributing factors: the availability of labor through migration and the shift from agriculture to higher productivity employment in industry; the institutional basis for labor relations and their role in wage restraint; and the part played by the state in directing investment and encouraging international trade. The Marshall Plan and the U. S. underwriting of European security eased budget constraints, increased investment, imports, and industrial growth, and sped the relaxation of postwar economic controls. Although none of the elements in this account are new, Eichengreen provides a masterful synthesis, explaining the general concepts underlying the strong economic growth and giving specific detail on individual country experience (classed according to the strength of growth from Germany as "pacesetter" to "laggards" including the United Kingdom, Ireland, and Belgium).

Given its exceptional character, particularly in closing the productivity gap by borrowing existing technol-

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ogies, the rapid postwar growth in Western Europe could not be sustained indefinitely. Increasing inflationary pressures and slowing productivity growth reduced the pace of economic expansion even before the oil shocks of the 1970s. Eichengreen's account of the era of slow growth is less detailed, emphasizing the difficulties of intensive, innovation-based growth in Europe after the great success of catch-up and convergence. He gives fuller attention to international payments and the context and logic for the European Union's movement to a single market and then to a single currency among the European Monetary Union eleven. He also provides a chapter with a brief account of the economic stagnation in the Eastern bloc, devoting greater space to the structural reforms, liberalization and privatization of the 1990s.

The breadth of coverage for the period 1945 to 2005 requires compression, and there are occasional errors on matters of detail. Eichengreen's previous scholarship on monetary and financial history informs his thorough coverage of these areas, and his interest in political economy yields close attention to the important role of politics, state policy, and cultural preferences in determining the path European countries have taken from 1945 to the present. The most notable gap is in the fleeting attention given to the evolution of the welfare state in Europe, which Eichengreen mentions only briefly for its impact in reducing the potential for recovery after the slowdown of the 1970s. Further consideration of the changing role of the state in European economic management over the past fifty years, assessing the benefits as well as the costs of state provision of education, health care, pensions, and public goods such as efficient public transit, with their relevance to cultural preferences and structural rigidities, would add to the book's value as a guide to contemporary European economic performance and prospects.

This is a landmark volume, by far the best available synthesis explaining European economic history since 1945, offering pertinent comparisons to U. S. experience that respect institutional differences and cultural preferences between countries. Its explanations and analysis are clear, concise, and engaging. Readers wishing more detail on the economic debates and national economic experiences covered will appreciate the state-of-the-art bibliography. Don't miss it.

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ALAN CROMARTIE. *The Constitutionalist Revolution: An Essay on the History of England, 1450–1642.* (Ideas in Context, number 75.) New York: Cambridge University Press. 2006. Pp. x, 309. $90.00.

It is hard to do justice to this rich and complex book in a short review; but in broad terms it can be summed up as describing a cultural transition in which ways of thinking about politics came to be reshaped by what Alan Cromartie calls "the rise of the common law." The "constitutionalist revolution" of the title was the assertion of common law as "the criterion of authority" by which Charles I's policies were judged in the first nine months of the Long Parliament. The stress placed on this is intended to counterbalance the emphasis on religion, and religious causes, as the driving force behind side-taking and division at the outbreak of civil war. As the author points out, one of the main problems with this thesis is that it presumes that religion can be isolated as a separate form of motivation. All too often in the imaginations of contemporaries concerns about religion, the common law, tyranny, and arbitrary government "leaked" into each other: for example, in the outcry caused by the canons of 1640 and the et cetera oath, which were presented not only as an attack on godly ministers but also as a legal challenge to the royal supremacy and a manifestation of clericalist tyranny.

Cromartie begins his account of the "rise of the common law" with Sir John Fortescue in the late fifteenth century, demonstrating that while there existed a deep-seated respect for the rule of law amongst the political elite, most of the political concerns and clichés of the period were about the need for "good counsel." This changed in the course of the early sixteenth century, when Christopher St. Germain developed the highly influential notion that the natural law could be equated with "common law reason" and therefore that the maxims and principles contained in the legal year books carried the same authority as statute law. Out of this developed the claims made by Sir Edward Coke in the early seventeenth century that common law wisdom was in effect a universal science, within which could be found legal answers to every question that was likely to be posed. The common law came to be seen as supplying the fundamental principles on which was based the welfare of the "common weal." Hand in hand with this development went the crown's increasing use of statute and law to legitimate the central changes of the Reformation. The Henrician statutes that enshrined the royal supremacy were followed by a raft of legislation from the Edwardian and Elizabethan regimes that showed a new respect for the rule of law. This "legalistic Reformation" strengthened the crown in the sixteenth century, but by establishing the omnicompetence of common law it had the effect of stiffening resistance once royal policies appeared to conflict with the rule of law under the early Stuarts. One of the clearest markers of this shift was the way in which forced loans were treated in the two periods. Under the early Tudors opposition was generally sporadic and based not on the illegality of the measures but on concerns about whether the king had been properly counseled in resorting to them. By the 1620s, however, they were seen as a fundamental infringement of the individual's property rights enshrined in law.

This is a wide-ranging and illuminating study. Much of the ground it covers is familiar. But the accounts it offers of topics such as the rise of a common law culture built around year books and the Inns of Court, the legal education of the magisterial classes, and the anticleri-

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calism of the legal profession are lucid and informative; and it pulls together material from a wide range of sources to offer an excellent account of the importance of common law within contemporary political culture. Its basic argument about the importance of not losing sight of the legal and secular case against Charles I amongst the causes of the civil war is well made; and as long as the temptation is avoided to elevate this to the status of a single, primary cause—as has tended to happen with claims made about "England's War of Religion" or, more recently, the importance of "Noble Revolt"—the thesis is highly persuasive.

RICHARD CUST
University of Birmingham

KEN MACMILLAN. *Sovereignty and Possession in the English New World: The Legal Foundations of Empire, 1576–1640*. New York: Cambridge University Press. 2006. Pp. xiv, 235. $90.00.

How did the English justify their right to govern and settle the New World? What arguments did they use to establish colonies on land that was invariably claimed by others, and what were the implications of those arguments for the subsequent history of English and, eventually, British America? As Ken MacMillan notes in the opening pages of this important new book, such questions have long interested British and American historians, yet few have written about them with the clarity and insight that MacMillan achieves here. Drawing on a broad array of sources, including maps, travel narratives, colonial charters, promotional tracts, and diplomatic papers, MacMillan is chiefly interested in two problems: first, the legal arguments with which the English sought to gain the recognition of Europe's other maritime powers (especially Spain and France) for their right to settle in the New World, and second, the precedent that this effort created for the extension of royal and (after 1649) parliamentary sovereignty over the far-flung inhabitants of England's Atlantic empire. Carefully argued and thoroughly documented, the result is a significant contribution to English and American legal and constitutional history, as well as to the field of British imperial history and the history of international law.

At the heart of MacMillan's analysis lies the distinction—familiar to legal historians and historians of the law of nations—between the crown's right to absolute imperial sovereignty (*imperium*) and the right to possess and rule territory under the crown's jurisdiction (*dominium*). As MacMillan correctly observes, British and American historians have generally focused on the second of these, especially the right of English settlers to exercise *dominium* by possessing territory claimed by Indians and other Europeans in America. The reasons for this tendency to emphasize a settler-based *dominium* over the *imperium* of the crown are not hard to find. For at least sixty years after the first permanent settlement at Jamestown (1607), England's presence in the western Atlantic was sustained almost entirely by adventurers and settlers who, despite holding charters and commissions from the crown, operated as autonomous agents. Although metropolitan supervision increased from the Cromwellian ascendancy of the 1650s onward, the crown initially played a relatively minor role in England's expansion, meaning that its imperial sovereignty depended to an unusual degree on the *dominium* of English settlement. Because the crown's authority over its indigenous American subjects was no more extensive, the history of native dispossession has generally followed a similar trajectory, with scholars emphasizing the instrumental role of a theory of *dominium* based on the right of agricultural settlers to possess and cultivate land that belonged to the Indians but that they invariably depicted as unoccupied and unimproved.

In the face of these trends, MacMillan argues that the *imperium* of the crown was in fact a centrally important part of England's expansion, starting with the North Atlantic voyages that Martin Frobisher made between 1576 and 1578. According to MacMillan, two factors account for this centrality. The first involves the use of Roman legal theory to legitimate England's Atlantic empire in the eyes of Europe's other colonizing powers. Although a theory of empire based on the *dominium* of occupation by English settlers made some sense under English common law, Roman law—the lingua franca of European diplomacy—was much friendlier to rule by prerogative, and it was hostile to any theory of empire that did not vest *imperium* in a unitary, absolute sovereign. Because England could only win international recognition by conforming to this model, the imperatives of European diplomacy acted as a theoretical check on the *dominium* of English settlers. No less important, English jurists used Roman legal theory as the preferred language of imperial government to ensure that the crown retained absolute sovereignty—*imperium* as well as *dominium*—over all lands and peoples under its jurisdiction. In a sense, the imperatives of foreign and imperial policy converged, with the servants of Elizabeth I and her Stuart successors making widespread use of Roman law both to uphold the crown's rights vis-à-vis its own subjects and to safeguard them from European rivals.

All in all, MacMillan makes a strong case for the place of Roman legal theory in the early stages of England's overseas expansion and for the justification that this theory supplied for a crown-based *imperium*. As the American challenge to the sovereignty of the British crown and Parliament would eventually show, there were limits to the centralized *imperium* that Roman legal theory tended to favor, by 1776 if not much earlier. Given the chronological parameters of his book, MacMillan largely avoids the question of when or how the colonial subjects of the English crown (and its British successor) began to modify the legal foundations that he delineates. Still, if he leaves some questions unexplored, there is no question that MacMillan has written

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a book of considerable importance, one from which British and American historians alike will benefit.

ELIGA H. GOULD
University of New Hampshire

DAVID FARR. *Henry Ireton and the English Revolution*. Rochester, N.Y.: Boydell Press. 2006. Pp. ix, 277. $90.00.

As I am writing this review of one of its protagonists, readers of a British broadsheet newspaper congratulate themselves on rescuing "the Putney Debates"—at which, in 1647, supporters of "wild or vast motion(s)" thrashed out a post–civil war constitution with other progressive but nervous Englishmen—from an obscurity into which historians did not realize it had fallen. One casual mention of Henry Ireton, subject of David Farr's monograph, confirmed his leadership of the cautious, along with the implication that he was the Machiavelli behind his father-in-law, Oliver Cromwell. Ireton possesses historical and historiographical features that bedeviled—often in literal embodiment—the son of the more famous father, and Farr starts with one reason why that should be: Ireton died before Cromwell came to power.

This denied Ireton a role as player within the Commonwealth, or architect of the Protectorate: maybe he would have been a protector, far more able that Cromwell's own son Richard. He was denied a hero's death, martyrdom, or a long, prominent career. Rather, he emerged as enigmatic as his father-in-law, for the opposite reason, and backgrounded like his diametrical opposite, republican anti-Puritan Henry Marten, although Ireton died a relatively honorable soldier's death while Marten's longevity consigned him to prison obscurity.

This presents Farr with problems spared students of Marten or Cromwell: the paucity of contemporary material and the neglect of historiography. Farr positions himself very clearly within a distinctive approach to both types of source. He proclaims himself a member of the "New Morrill Army": as a curious relic of this, a list of bibliographic abbreviations includes the usual references—*CJ* (*Journal of the House of Commons*), *CSPD* (*Calendar of State Papers Domestic*)—but also those to lesser contributions by the architects of the modern canon. In all, Farr does a fine job with the material at his disposal. His study is rigorous and scholarly, makes good use of a wide cross-section of manuscript and printed primary sources, and has delved headlong into the huge modern literature of the civil war period. He writes with flair and interest—no small matter when compiling a biography on conventional, chronological lines.

That said, there remain tantalizing skeins to unravel, and it is hard to know whether any scholar will weave a smooth cloth. Having patch-worked the relative provincial obscurity of the Ireton family in the east midlands, Ireton leaps into the center of parliamentarian politics without reference to Nottinghamshire and Derbyshire regionalism. Did Lucy Hutchinson appraise Ireton as a fellow from Nottinghamshire as well as an opponenther husband's more radical politics; and where did Ireton fit within the parliamentarian factionalism of Derbyshire, sandwiched between the Presbyterian Gells and the radical Saunderses (there is no mention of Thomas Saunders; was Robert Saunders [pp. 54, 61] related?).

Farr's second problem is with Ireton's reputation; his early death pushes the emphasis onto his politics during the civil wars and their immediate aftermath—that is, on the politics of conflict and uncertainty and not the politics of rebuilding and stability. Ireton is all too easy to reconstruct, through the pre-regicide vitriol of the Levellers or the post-Restoration satire of royalists, as the scheming, plotting, devious back-room operator. This may be a true picture, but the historian of Ireton is left trying to piece together what Ireton really meant, from what little he actually said. Maybe he was the original politico. Hence Farr's discussion of the Putney Debates tries to stand back from and position Ireton as a more open, facilitating figure than has previously been the norm. Ireton and Cromwell at Putney are described as preservers of consensus, and while they may have been articulators of a moderate position that warned participants to take heed of previous engagements, the implication is that they distanced themselves from the factionalism of both sides and the political philosophies that gave them form. What results is another articulation of authorial debates: did John Wildman write *The Case of the Army*? Did Ireton imply he did or he did not? Is to continue this debate to mask the real politics beneath? To nuance the radical (and the moderate) positions at Putney continues to relegate the degree to which Putney was a beacon of English constitutional liberty, but also further obscures, behind a tangle of obscurities, the thought of Ireton himself.

But an inability to unlock—or unlock fully—Ireton's mysteries does not diminish Farr's attempt. This book does not close the enquiries about Ireton's part in the revolutionary action of 1640s England, nor does it open up new fields. It makes a contribution and it does not diminish our fascination with the period, or Ireton's part within it.

SARAH BARBER
Lancaster University

ROGER WOOLHOUSE. *Locke: A Biography*. New York: Cambridge University Press. 2007. Pp. xviii, 528. $39.99.

Roger Woolhouse has written a fine new comprehensive biography of John Locke. Although Locke's political, economic, educational, and religious works were extremely important and influential, Locke was proudest of his *Essay Concerning Human Understanding* (1689). It is therefore appropriate that this biography is by a philosopher, and that it is at its best in clear, concise, and elegant analyses of Locke's epistemology, while providing good if brief analyses of all of Locke's major works. By discussing the contents of the huge vol-

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ume of papers and journals composed by Locke, Roger Woolhouse illuminates much about the intellectual origins of many of Locke's major works. A particularly fine sense is communicated of Locke's deep pragmatic interests in natural philosophy and medicine, and of their significant roles in his epistemology. The importance of issues of trade to Locke is delineated, leading to Locke's late in life lament that his head belonged "to a man of trade, and is thwacked with sea-coal and fuller's earth, lampblack and hob nails." A measured and useful account is given of the development of Locke's political and religious views on issues such as resistance to tyranny, faith and reason, and toleration. Locke's personal reaction against scholasticism and the educational regimes of Westminster and Oxford are linked lucidly to his *Some Thoughts Concerning Education* (1693). Judicious if brief discussions of works to which Locke responded directly or which responded directly to Locke's works flesh out a sense of the intellectual contexts within which Locke was working.

Woolhouse is especially effective at giving us a vivid portrait of Locke's life. He utilizes Locke's voluminous correspondence to present us with a complex human being who was a bookish and markedly sexist lifelong bachelor; a compulsively secretive and taciturn user of invisible ink and shorthand and a parsimonious and often irritable friend who yet had many affective ties and was an affable correspondent and conversationalist; a medical practitioner in almost permanent anxiety over asthmatic ill health and often called upon to live in "that sink of sin and sea-coal," London; and an intellectual deeply concerned with the political life of his nation who declared at one point that "I would be quiet and I would be safe" while asserting his willingness to take up arms to gain security by "strength and steel," who put considerable effort into avoiding a quiet, safe life of clerical obscurity, and who wrote arguably the most important and influential justification of resistance to tyranny ever composed. This rich portrait of Locke, suggestive of his inner strength and steel, is enhanced by imaginative use of Locke's unpublished manuscripts to highlight particularly telling moments or ideas—as, for instance, in indicating the significance to Locke of the trial for treason of the Whig propagandist Stephen College by using Locke's record of weather observations to show that he was up at 4:00 a.m. on the day that College's trial went far into the night, before rushing to London with news of the conviction that led to College's execution.

These are many and considerable achievements. The sheer volume and range of Locke's works, interests, commitments, and activities, and their now frequently contested interpretations, however, make it impossible to compose a biography capable of analyzing all topics and all of Locke's works in full detail in ways that will satisfy specialists in every modern discipline. This biography necessarily has its limitations. The concentration on Locke's life and his individual intellectual trajectory brings considerable benefits, but that trajectory might have been illuminated further by expanding analyses of Locke's texts, even at the cost of losing some of the entertaining details about the life, because it is the works that make the life worth telling, and their nuances, complexities, and limitations are sometimes inadequately explored. For historians, it would have been valuable to have drawn back the lens farther in order to give more of a sense of the debates in which Locke participated and of the scale of transformations and contestations in the times through which Locke lived, England's "Century of Revolutions." This work could have been richer in analyzing and contextualizing Locke's political and economic thinking. More emphasis could have been given to Locke's various forms of support for or challenges to hierarchy, coerced industriousness, and slavery. Delineating religious orthodoxy and intolerance more fully would have enriched appreciation of Locke's developing unorthodoxy and arguments for toleration, and more emphasis could have been given to the significance of legal threats posed by absolutism in analyzing the *Two Treatises of Government* (1689). The book has inadequate and irritating footnotes, and a number of minor errors have crept in. Nonetheless, this is a fine addition to the scholarship on Locke and will serve as a very useful account of his life and a good introduction to his thought.

JOHN MARSHALL
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PATRICIA SPRINGBORG. *Mary Astell: Theorist of Freedom from Domination*. New York: Cambridge University Press. 2005. Pp. xix, 372. $80.00.

In *Reflections upon Marriage* (1706), Mary Astell asks that "If *all Men are born free*, how is it that all Women are born slaves? as they must be if the being subjected to the *inconstant, uncertain, unknown, arbitrary Will* of Men, be the *perfect Condition of Slavery*?" In the past few decades, these words have resonated with modern liberal feminists who seek to extend the political ideals of full liberty and equality to women. For them, Astell's question is the rhetorical flourish in a simple but powerful argument: if all human beings are naturally free, then it is an injustice for any human being to be born into subjection; and it is therefore an injustice for any woman to be born in subjection to men. Other scholars point to the fact that, with these comments, Astell appears to support the republican ideal of liberty as freedom from arbitrary sway and domination.

Patricia Springborg uses Astell's famous rhetorical question as a focal point for her discussion. As her title suggests, Springborg regards Astell as a theorist of freedom from domination—but not in the modern, secular sense. Springborg proposes to offer an assessment of Astell's overall theoretical position—one that is distinctly at odds with modern liberal, republican, and feminist sentiments. For Astell, liberty is a spiritual rather than a political concept: it consists in the subject's choice to pursue or not to pursue that which is good for the soul, and it can be exercised by anyone, anywhere, regardless of external circumstances. For

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this reason, and without compromising consistency, Astell could be both an advocate of liberty for women and a supporter of passive obedience to authority in both the family and the state. As a Christian royalist, Astell thought that subjects ought to remain obedient to their rulers, however tyrannical or irreligious they might be—and she thought that wives should practice a similar obedience toward their husbands. Springborg shows that, far from affirming the republican ideal of liberty, Astell challenges Whig theorists to extend their unquestioning support for female obedience in the home to support for the passive obedience of subjects in the state. Placed in its proper context, Astell's famous question is part of a *reductio*: if all human beings are born free, and if women fall into the category "human being," then why is it that all women are born slaves? Evidently it is not the case that all human beings are born free; and clearly, for Whig thinkers such as John Locke (the target of Astell's remarks), it is acceptable for *some* human beings (women) to be subject to the arbitrary will or discretionary power of others. Why not others?

In her various chapters—on Astell's theology and philosophy (chapter one), education theory (chapter two), feminism (chapter three), and politics (chapters four through seven)—Springborg shows how Astell's works are informed by an unswerving commitment to the High Church Anglican and conservative Tory cause in early eighteenth-century England. In doing so, Springborg brings together material from at least seven previous publications on Astell, and there is little here that an Astell scholar would not have already read. But this does not detract from the fact that Springborg is a formidable scholar with an extraordinary depth of learning. Her account of Astell's political thought is truly authoritative: it is detailed, erudite, and textually and historically informed.

If there is one flaw with the book, it concerns the critical targets of Springborg's arguments. Springborg says that her motivation for collecting past material is to correct various misrepresentations of Astell's thought. But one wonders, first: who are the modern feminists who misrepresent Astell's views on women? Since Joan K. Kinnaird's 1979 article on "Mary Astell and the Conservative Contribution to English Feminism" (*Journal of British Studies*), most scholars endorse the view that Astell's feminist views are closely connected (rather than antithetical) to her political opinions. Few commentators (that I know of) are still wondering about "how to reconcile Astell's feminism with her High Church Tory politics" (p. 3). And, second: who are the republicans who have "claimed [Astell] as their own" (p. 5)? Springborg singles out Philip Pettit as someone who has wrongly identified Astell as a republican thinker (p. 1). But in *Republicanism: A Theory of Freedom and Government* (1997), Pettit simply says that Astell's famous remark is appealing to feminists who subscribe to the republican ideal of liberty. With explicit reference to Springborg herself, he allows that Astell's own "rhetorical intentions in making this remark are complex and difficult to discern" (p. 139); he never says that Astell is a republican. In the absence of further details, one is left wondering if some of Springborg's targets are rhetorical inventions.

It remains, however, that Springborg's research has played—and with this book, will continue to play—a crucial part in igniting sophisticated critical debate about Astell's writings. Astell's biographer Florence Smith once observed that Astell's political works had passed into oblivion alongside the party controversies of her day. Springborg has not only rescued those writings from oblivion but established their significance for Astell's thought as a whole. Springborg has performed an inestimable service for Astell studies and for the study of early modern women philosophers in general.

JACQUELINE BROAD
Monash University

JANE SHAW. *Miracles in Enlightenment England*. New Haven, Conn.: Yale University Press. 2006. Pp. x, 244. $45.00.

A fascinating study of English miracle cases between 1650 and 1750, Jane Shaw's book delineates three streams of thought and practice among English Protestants regarding miracles in this period. Across eight chapters (including the introduction and brief conclusion) Shaw shows how Protestants in sixteenth and early seventeenth-century England came to argue for the cessation of miracles after the time of Christ's apostles; that nevertheless, in the seventeenth and early eighteenth centuries, members of various groups claimed to perform or to have experienced miracles; and, finally, how some used the approach of experimental method to carve out a *via media* between extreme rationalism and enthusiasm. Chapter two records how the doctrine of the cessation of miracles developed "out of the particular context of the sixteenth-century conflict between Protestants and Roman Catholics" (p. 23) in England, and how miracle claims began to re-emerge during the English Civil War and Interregnum, particularly among Baptists and Quakers. Chapters three through six explore such claims in depth. Chapter three examines the political dimension of healings by Quakers in the 1650s alongside the reassertion, after the Restoration, of the monarch's claim to heal scrofula (the King's Evil) by touch. The captivating fourth chapter addresses the "Irish Stroker" Valentine Greatrakes and the Royal Society luminaries of the mid 1660s who observed and even participated in his healings. Chapter five studies Martha Taylor's equally investigated claim in the same decade to have survived eighteen months without food, in the larger tradition of female prophecy and fasting. The sixth chapter examines Marie Maillard's spontaneous healing while reading Scripture in 1693, alongside accounts of similar miracles, in the context of the "political, religious and cultural task of the 1690s . . . to form a strong and safe Protestant nation" (p. 126). The last full chapter discusses the way skeptics and deists challenged all three of these Protestant approaches to

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miracles in the first half of the eighteenth century, ultimately posing the questions of whether the Christian God could or would work miracles at all.

The key contribution of this volume is a rich contextualization of the debate on miracles—familiar from the writings of David Hume and Bishop Butler—by relating it to lived religious experience: "it is no surprise," Shaw rightly tells us, "that a half-century of miracle claims and the debates they generated, in the latter part of the seventeenth century, should be followed by a philosophical debate on miracles" (p. 13). She shows that this debate took the shape and prominence it did for Royal Society fellows, theologians, and their adversaries quite simply "because so many people were making claims that they had experienced miracles or were able to work them" (p. 15). Through clear prose and circumspect claims, Shaw situates these miracle cases at the heart of the Habermasian emerging public sphere. She shows how they were "enabled and enhanced by the new media of the Enlightenment" (p. 5), investigated by Royal Society luminaries, debated in print, and expostulated upon in pulpits and coffee houses. Shaw's monograph is an important contribution to scholarship that, following J. C. D. Clark's *English Society, 1688–1832* (1985), challenges conventional assumptions about the decline of religious belief in eighteenth-century England. "In short," Shaw tells us, "religion was shaped anew in the Enlightenment, and was central to the whole project of modernity" (p. 17).

It is on this point that one might wish for Shaw to address the historiographical and sociological implications of her study more directly. If religion in the Enlightenment made itself anew, what if anything constitutes the essentially religious as a subject of study? In place of debating the secularization thesis, can we speak, however paradoxically, of more and less secularized religions? If so, the very definition of secularization comes in for an equally dramatic overhaul. Shaw aligns herself with sociologist David Martin's argument that "instead of regarding secularization as a once-for-all unilateral process, one might think rather in terms of successive Christianizations, followed or accompanied by recoils" (*On Secularization: Towards a Revised General Theory* [2005], p. 3). But all Christianizations are not equal. These spectacular and debated miracle stories—and the spiritual lives to which they testify—differ signally from Christian miracle stories in other times and places. In them we are not asked to understand the nature of God or holiness more deeply, as in the miracles of the Gospels, *The Sayings of the Desert Fathers*, or the *Life of Saint Anthony*. Shaw's book helps us see the way Christianity in seventeenth and eighteenth-century England was refashioned in the terms that mattered in its day: hard evidence of natural laws and facts. From the rich flowers of its first millennium, Christian spirituality in modernity is reduced almost solely to external spectacle and internal confirmation of whether it is true or whether the spiritual even exists; as J. G. A. Pocock and others have pointed out, enthusiasm and enlightenment are two sides of one proverbial coin ("Enthusiasm: The Antiself of Enlightenment," in Lawrence E. Klein and Anthony J. La Vopa, eds., *Enthusiasm and Enlightenment in Europe, 1650–1850* [1998], pp. 7–28). One needs ears to hear what is lost when Christianity becomes the end of a syllogism, a system of knowledge like several other ways of ordering the world in this period. Through accounts of so many believers anxiously seeking miracles as proof of the validity of their religion (which would paradoxically then lose the status of faith—a question not much taken up in the period or by Shaw), these chapters raise the question of just how possible it is, within an increasingly naturalistic mindset, to believe in a miracle as a religious event at all, even after witnessing what one sees as an inexplicable healing. Shaw's words about Martha Taylor speak to her other examples, highlighting the inevitability of acts of belief in all production of knowledge: "she was whatever those who interpreted her made her out to be: a wonder, a providential sign to a sinful nation, an impostor, a purely medical phenomenon" (p. 104).

LORI BRANCH
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LORI BRANCH. *Rituals of Spontaneity: Sentiment and Secularism from Free Prayer to Wordsworth*. Waco, Tex.: Baylor University Press. 2006. Pp. xiii, 348. $39.95.

Spontaneity increased in significance and became a sign of sincerity not only in religion, but also in many forms of literature, moral philosophy, economics, and the wider social discourse in Britain in the long eighteenth century. So argues literary critic Lori Branch in this important and lively book, full of interesting ideas and interpretations, which focuses on the importance assigned to spontaneity in a range of texts from John Bunyan to William Wordsworth.

Branch begins her story with the turn to free prayer among Protestants in the seventeenth century. Over and against the liturgical forms of the established church's Book of Common Prayer, Puritans and dissenters presided over what Branch describes as "the most dramatic sea change in 2,000 years of Christian worship and practice" (p. 40). While there may be other contenders for this claim, certainly we can agree that Puritans and Independents alike privileged an emotional experience of the Holy Spirit and the practice of extemporaneity in their prayer lives. Pamphlets and texts paradoxically prescribed spontaneity and sentiment; Bunyan's struggle with praying and writing from the heart highlights these paradoxes. This seventeenth-century religious discourse on the value of spontaneity was transformed into a secular one, argues Branch; writers such as the earl of Shaftesbury, Oliver Goldsmith, Christopher Smart, and Wordsworth translated formerly religious values into economic and moral terms.

There is, however, a bigger story at stake in Branch's work than simply the rise of spontaneity in these discourses. Her narrative repeatedly points to the emer-

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gence of a secular/religious binary during this period of the Enlightenment. Historians have in recent decades emphasized the ways in which religion, far from being thoroughly "rationalized" or even eroded in the eighteenth century, was thriving in many ways, and underpinned numerous intellectual and cultural debates. Branch's analysis builds on this work to some extent, although she is self-consciously writing for a literary audience, which, she claims, quoting Paula McDowell, "remain[s] strikingly unaffected by these developments" (p. 219). Branch goes on to argue that the secular models of intelligibility and value, at the heart of current literary and cultural criticism, ignore the innumerable religious references in eighteenth-century texts. It is these references, and the logic behind them, that she attempts to bring to the surface of her readings. She points to the ways in which Shaftesbury invented a deliberately secularized notion of "true enthusiasm" while at the same time imbuing his writing with the logic and language of free prayer. Religious discourses are therefore simultaneously denied and retained in his moral philosophy. In Goldsmith's work and life, she detects a tension between the desire for goodness and the realities and mechanisms of eighteenth-century commercialism, exemplified in what she describes as the "secularized" form of religion, with its emphasis on sensibility and emotion, in *The Vicar of Wakefield* (1766).

Historians and sociologists have debated the secularization thesis for several decades, some questioning it wholesale—precisely because it does not adequately describe our conditions in the present day or the eighteenth century—with others wishing to keep the secularization paradigm in place but pointing to its complexities. Branch belongs to this latter group: she takes secularization as a given but borrows from a range of theoretical works not only to redefine it, but also to refigure its very process. For her, "the story of spontaneity . . . is the story of secularization of goodness" (p. 4); this is not a smooth process, nor one that is external to religion, but one that takes place from within, as religious thinkers embrace forms of rationalization. The rise of spontaneity and the rationalization of religion therefore go hand in hand, in eighteenth-century Britain.

Branch's contribution to the debate is a valuable one, though firmly fixed on texts and ideas rather than practices and expressions of faith. It is in the final chapter that the full ambitions of her project are articulated. While poststructuralist critics have undone a particular Enlightenment dream of objectivity and rational certainty, they have continued to ignore the place of religion, unquestioningly retaining this legacy from modernity. There is an irony in this, argues Branch, who suggests that the logic of the postmodernist position should lead to the declaration of "faith" (broadly conceived) and thus to a more vulnerable stance in scholarship, precisely because postmodernists call us to acknowledge—as Smart put it—that we "speak ourselves" from the crown of our head to the soles of our feet. Not all historians will like the long engagements with poststructuralist theory, especially in the first chapter (though Branch herself suggests that some readers might want to head straight for chapter two); but the final chapter is a heartfelt appeal for humility, joy, and spontaneity in scholarship. Whether everyone would call that faith, as Branch does, is another matter, but there is no doubting the sincerity of Branch's plea.

JANE SHAW
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JEREMY BLACK. *George III: America's Last King*. (The English Monarchs.) New Haven, Conn.: Yale University Press. 2006. Pp. xvii, 460. $35.00.

Do we need another book on George III? The trouble with political biography is that its wider relevance to events and ideas depends greatly upon the extent to which its subject was an independent and influential actor in them. In this lively and accessible contribution to Yale's "English Monarchs" series, Jeremy Black convinces us that George III had a keener political mind than most of his predecessors and that, health permitting, he played an active, dutiful, and highly principled role in government. Unlike his Hanoverian predecessors, he exploited his Britishness, distanced himself from his European roots and embraced the role of "patriot King," unconcerned with the factional squabbles of the Whig ascendancy. The confluence of majesty and Protestant national identity has become familiar to us over the last two decades through the work of Linda Colley in particular, but whereas Colley's interest has been with the role of monarchy as a factor in the "forging" of image and identity, Black's book looks more deeply into the soul beneath the crown.

Despite its subtitle, this is not a book devoted to the reassessment of colonial failure, for it analyzes the entire sixty years of George's eventful reign in some considerable detail and makes use of a prodigious and varied range of manuscript sources to do so. Nevertheless, the American debacleremains central, for in Black's view, the republican experiment represented the eighteenth century's only "major new political departure in the English-speaking world," and George III was paradoxically the nearest any leader of the old order came to emulating the leaders of the new, for, with George Washington, he "shared a commitment to public virtue based on duty, integrity and steadfastness." While for many of George's earlier biographers these traits amounted only to stubbornness and, for his American adversaries, to outright tyranny, Black is anxious to rescue the king from teleological reproach. Rejecting charges of tyranny may be justified, given a time when other European thrones were still characterized by theories of absolutism and divine right, but George's own patronizing and paternalistic misunderstanding of the rebels as "penitent children" nevertheless suggests a serious inability to comprehend the intellectual currents in which he swam. Black notes the tendency for American propagandists to accuse George personally of tyranny rather than his ministers, but this, as even remonstrating English Paineites would understand a couple of

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decades later, was a constitutional necessity for the discursive justification of popular resistance.

We may of course question the extent to which any monarch is really worthy of too much personal historical fuss. There can be no doubt that the monarchy went through some extraordinary evolutionary changes during George's reign, but the extent to which these were influenced by the court remains an open question. George did, as Black reminds us, depart from established royal practice in a number of ways; mixing personally with his subjects, showing himself off through constant travelling (in southern England at least), and becoming a recognizable pubic figure, even to the majority who never saw him, through the widely circulating and ambivalent caricatures of James Gillray, Richard Newton, and others (regrettably, very few are reproduced in the book). George visited manufactories, patronized charities, took a public interest in science and agricultural improvement, and endorsed the new Royal Academy of Arts. He (or those around him) made much not only of his institutional patriotism but of his cultural Britishness, too, and his ordinariness, solicitous approachability, frugality, and domesticity were favorably contrasted to the lifestyle and character of the autocrats across the channel.

As one might expect, Black's book is by no means a cultural history of monarchy but a carefully argued political biography, and it takes only passing interest in issues of "spin" and the cultural construction of forms. In placing a single individual at its center and understanding the unfolding of events through his singular experience, the approach is largely a traditional one, unfolding a progressive narrative between the "first years" and "last years" of the reign. But in some ways it is refreshingly nontraditional, too. Black breaks up his chronology with a series of judicious and critical chapters on contextual themes: "Character and Behaviour," "Family," "Religion and Morality," and so on.

There is a particularly useful and timely closing section, beginning with an historiographical survey of shifting moods among the king's biographers from Horace Walpole to J. H. Jesse, G. M. Trevelyan, J. H. Plumb, John Brooke, J. R. Christie, and even, most recently, the current Prince of Wales, moving on to an attempt to situate George among his European peers, and concluding with some thoughts on the reign's contribution to the making of the modern monarchy—most significantly perhaps in the "increased popularity of the crown amongst the middling sort of people." George was, Black points out, somewhat fortunate to have reigned and survived during an age of European revolutionary upheaval, profound social and economic change, and rapid industrial, scientific, and technological development. His personal popularity benefited from public sympathy over his illness, from public disdain for his successor, the "largely purposeless" Prince of Wales, and from his stoical handling of various bungled assassination attempts. But on the world stage, it benefited too from the contiguous rise of the British Empire and from the attempts of ministers in the 1790s to rebuff reform and revolution by coalescing the nation under a pretense of faction-free, mild, and happy government.

This is not only a well-written biography from the restless pen of a prolific political historian; it is also perhaps the most thorough and meticulously researched biography of George III to date.

Steve Poole
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F. P. Lock. *Edmund Burke: Volume II, 1784–1797.* New York: Oxford University Press. 2006. Pp. xiv, 605. $160.00.

This imposing second volume brings F. P. Lock's meticulously researched biography of the eminent eighteenth-century statesman and writer to a close. It is devoted primarily to the two great crusades of Edmund Burke's later political career: the sustained campaign against the abuses of empire in India in the 1780s, and Burke's epochal intervention against the French Revolution, launched by the publication of *Reflections on the Revolution in France* in 1790. These are perhaps Burke's main public achievements, but they are not the only landmarks of his variegated life. Burke, the gentleman farmer who struggled to make ends meet at Beaconsfield, is of as much interest to Lock as the statesman in Whitehall, and not least of the merits of the attention to detail throughout this book is how much it fills in of the rare glimpses of Burke's personal life, hitherto provided only in his letters.

Burke presents an unusual problem for historical biography, however, for unlike most practicing politicians, he was also a political philosopher and an aesthetic theorist of note. His early *Philosophical Enquiry in the Origins of Our Ideas on the Sublime and Beautiful* (1757) launched a revolution of its own in eighteenth-century criticism. The biographer thus faces at the outset the difficulty of negotiating the "maladjusted relationship between philosophy and history" that, according to J. G. A. Pocock, has vitiated much of the history of both politics and political thought. The disparate and often elusive facts of Burke's life lend themselves to the kind of intensive empirical research at which Lock excels, but what are we to say of Burke's ideas, not to mention his extraordinary way with words, the sources of much of his contemporary reputation?

One of the more valuable aspects of Lock's approach is to contest the old canard that Burke's command of rhetoric was to the detriment of truth, and that his passionate intensity in both speech and prose came at the expense of precision. Such was the theatricality of Burke's performance in the House of Commons during the impeachment of Warren Hastings that Mrs. Siddons, the great tragedienne, is reputed to have fainted, but as Lock points out, such dramatic bravura was punctuated by long discursive tracts of factual exposition and statistics, often backed up by intensive research. In what is perhaps the dramatic tableau of his own long

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narrative, Lock demonstrates that Burke's famous panegyric to Marie Antoinette in the *Reflections*, lamenting her cruel violation "by a band of cruel ruffians and assassins," was based on factual sources, or such as were known to Burke. Lock rightly takes issue with critics of a literary bent who equate rhetoric, theatricality, and even Burke's romance of chivalry with "fiction," as if these were not also to be found in everyday life.

It is at this point that Lock's own method lets him down, however, for while he is willing to reconcile Burke's rhetoric with the real world, he fails to recognize the same may be true of Burke's aesthetics and, in particular, his concept of the sublime. The issue is of more than passing interest, for if Burke in the *Philosophical Enquiry* favors the aesthetic intensity of the sublime over the more demure pleasures of the beautiful, how are we to account for his negative response to the French Revolution—the political apotheosis of the sublime? It is perhaps for this reason that Lock is reluctant to use Burke's own key concepts of terror, pain, sympathy, and contagion, as expounded in the *Enquiry* and elsewhere, to negotiate his responses to revolution and empire. Lock consistently plays down the manner in which sublimity and the Gothic were not mere literary devices but "equipment for living" (to cite another Burke) in dark times.

It is perhaps no coincidence that the displacement of the sublime also brings with it a marginalization of Ireland in Lock's account of Burke. It is true that Burke's traditionalism, and homage to the ancient constitution, have more in common with the beautiful (or, perhaps more accurately, the picturesque) in a British context, but when it came to Ireland, Burke's sympathies were infused with the sublime. Lock omits any discussion of the overt Gothic register of Burke's remarkable letter to his son Richard in February 1792, where he warns of attempts by the Protestant Ascendancy to disturb the restless ghosts of Irish history. More inexplicably, there is no discussion of any of Burke's unguarded letters to his friend, the Reverend Thomas Hussey (who merits only two mentions in the volume), in 1795, where he advocates the arming of Catholic insurgent movements, such as the agrarian "terrorists' " the Defenders, against sectarian Protestant pogroms: "Dreadful it is; but it is plain enough, that Catholic *Defenderism* is the only restraint upon Protestant *Ascendancy*."

In its comprehensiveness and eye for detail, Lock's biography is sure to become the definitive life of Burke, but in keeping with the unfinished and obscure nature of the sublime, it is far from being the last word on the subject.

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Nicholas Tracy. *Britannia's Palette: The Arts of Naval Victory*. Ithaca, N.Y.: McGill-Queen's University Press. 2007. Pp. xii, 476. $65.00.

In a welcome effort to bridge the two solitudes that are naval history and art history, Nicholas Tracy, author of several books on strategic dimensions of British naval history, has written a study of the British "war artists" of the 1793–1815 conflict with France. His book offers itself as the collective biography of an artistic "band of brothers": painters who lived through the years of the conflict and made it the subject of their work. Although the product of substantial archival research, ultimately this bookfails to advance historiographical debate in either of its originating fields.

The first chapter, "Artists and the Navy," is broadly introductory. Tracy outlines the various period art genres (the sublime, the picturesque, history painting) that influenced what he calls "the arts of naval victory" (defined catholically as any painted representation of a naval action, naval event, or naval vessel). The author supplies a good deal of background information on artistic life in eighteenth-century London, along with some general prefatory information on the cohort of artists. He raises some interesting topics, such as whether the period saw the rise of sympathetic representations of the common seaman, the tension between the accuracy of historical representation and the exigencies of artistic composition, the link between marine painting and intelligence gathering, and the cultural impulses that lay behind the demand for these paintings in the first place.

Unfortunately these issues are not sufficiently explored in the nine chapters that follow. Tracy adopts a chronological narrative and his book is primarily descriptive in its content. Chapters proceed through the war, highlighting various artists and their works as the occasion demands. Thus, the second chapter describes the artists and paintings of the "Glorious First of June" (Admiral Howe's victory over the French fleet of May 29–June 1, 1794), with Philip James de Loutherbourg, Mather Brown, Robert Cleveley, and Nicholas Pocock receiving featured biographical discussion. Other chapters unfold similarly. The third focuses on the battles of St. Vincent (February 1797) and Camperdown (October 1797); the fourth, on the Nile (August 1798) and Copenhagen (April 1801); the fifth, on Trafalgar (October 1805), and so on to 1815. This arrangement gets complicated, however, for the simple fact that so many artists painted multiple battles. This forces Tracy to juggle his biographical ordering and to even out the chapters with sketches of distinctly minor artists (and minor actions). Befitting his specialization as a naval historian, Tracy supplements the wartime chapters with concise explanations of the strategic contexts of the naval actions surveyed. These are very well done. Four chapters on the postwar period, grouped more topically (with one devoted exclusively to J. M. W. Turner), round out the study.

The central problem with this book concerns its organization. The chapters unfold as serial biographies, and slavishness to the chronology of the naval war obscures the arguments or analyses that might otherwise have been developed. The problem is acute because Tracy justifies his study as necessary due to the disparate nature of the existing biographical literature on the

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individual artists. His solution does not adhere to the methodology of collective biography as usually practiced by historians. What he presents are a series of biographical treatments. However, the shared characteristics of the collective cohort are never systematically examined. The biographical sketches themselves are excessively detailed, overly prone to unwarranted speculation, and tend toward an excessive use of block quotations (usually from primary sources). Tracy is very attentive to the personal lives of the artists he chronicles, but that attention comes at considerable cost.

It is difficult to review a book that advances no sustained argument or thesis. Ultimately the question becomes, "to what audience is *Britannia's Palette* addressed?" Although published by a university press (and thus presumably seeking an academic audience), this work is of limited value for specialists. Tracy is an enthusiastic researcher, but because he privileges chronology and description, he rarely engages with the scholarship of others. In his introductory chapter, Tracy offers that "the artists who portrayed ... the navy played a central role in its history by the effect they had on public opinion" (p. 5). Precisely what image of the navy was presented, and what purposes it might have performed, are never examined in this book. A general readership interested in naval history, or art collectors drawn to the plethora of painter biographies, are the likely beneficiaries of Tracy's labors.

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Christopher Leslie Brown. *Moral Capital: Foundations of British Abolitionism*. Chapel Hill: University of North Carolina Press for the Omohundro Institute of Early American History and Culture, Williamsburg, Va. 2006. Pp. x, 480. Cloth $55.00, paper $22.50.

Christopher Leslie Brown's book is a crucial intervention in our understanding of the international pressures that led to the generation of that complex of ideologies and individuals formularized under the term "British anti-slavery." In its methods, its majestic scholarship, its lean and clean style, its patience, and its great common sense this meditation on the vastly complex social and intellectual origins of British anti-slavery activism takes us back to basics, and asks radical questions that historians of the Atlantic diaspora will now need to ponder.

The book revolves around an attempt to explain a single phenomenon: why did the movement advocating the abolition of the British slave trade occur exactly when it did, why did it so rapidly become both general and influential, and how did it achieve its ends so quickly? These simple and central questions had, before Brown's study, never been adequately addressed, and they are kept in unwavering intellectual focus despite the necessary intricacies of the secondary and tertiary lines of argument Brown follows.

It is particularly apparent in 2008, as British culture has erupted around the commemoration of the bicentennial of the 1807 Slave Trade Abolition Act, that Brown is taking on one of British history's most mythologized, and socially well protected, philanthropic narratives. The book consequently has to start with some dramatic clearing operations. The introduction is rather heroic, given that Brown must locate what he is doing in relation to some spectacular ideological and historical quagmires. The inherited historiography generated by British abolition 1780–1807 presents a contested and conflicted terrain. Brown consistently rises above the detail and sets his central ideas within the bigger scholarly picture. I will give just one example of his abilities as historiographical landscape artist: his analysis of Thomas Clarkson's *History of the Rise, Progress and Accomplishment of the Abolition of the African Slave-Trade by the British Parliament* (1808). Brown isolates for the first time the unique influence of this enormous work, published on the heels of the passage of the 1807 Act. The *History* still constitutes our primary source for the day-to-day workings and structural development of British anti-slavery, but its impact for Brown is primarily in enabling a set of hugely influential subsequent narrative paradigms. The analysis of Clarkson and his book in the seventh chapter articulates how it laid down the structures in which the received history of the movement were to develop, and from which anti-slavery would not and could not escape. Clarkson's thesis was essentially religious, and providential. Abolition could only be finally explained as the result of a divine intervention, with Britain chosen as the site for a near miraculous moral awakening. Brown sees this position, despite various vicissitudes, as remaining remarkably cogent and coherent within Anglo-American thought well into the twentieth century.

Brown's basic premise is that the protean origins of British abolition have been hugely neglected. He argues that anti-slavery has been, and continues to be seen as, a monumental force that suddenly arose more or less perfectly formed in the late 1780s, as the result of a powerful philosophical, religious, and moral awakening. In this sense the Clarksonian (and indeed Clapham sect) explanatory models have not fundamentally shifted beneath the historical narrative accretions they have gathered. Brown boldly refuses this inheritance, and goes so far as to announce the stark truth that "the British anti-slavery movement was an historical accident, a contingent event that might just as easily not have happened" (p. 30). This is shocking stuff, and a fine example of Brown's ability to force us to see the familiar with new eyes. In this book British anti-slavery emerges as a sensitive plant growing out of precarious and contradictory conditions, many of them generated in indirect, uncontrollable, and unforeseen ways by the American Revolution.

The American Revolution is central to Brown's meditation on the early evolution of anti-slavery thought. When I picked up this book, I considered the subject was more or less closed; there was nothing to be said that could shift the fundamental premises by which the development of the first wave of British abolitionism

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was explained. But Brown has thought so deeply through and around his subject from an Anglo-American perspective that he recreates it for us as though it were a new world. The American Revolution is shown to have inflected the social, moral, and political construction of slavery at a variety of levels in English intellectual and moral life. What finally emerges is the precariousness and fortuitousness of the British anti-slavery campaign. I particularly liked Brown's willingness to play imaginative games with history. Question: what if Quaker-led anti-slavery had climaxed in London not in 1788–1789 but in 1791? Answer: the San Domingue slave revolution would have buried it. He convinces me that the anti-slavery movement could not have occurred without the multivalent impacts of the American Revolution by coming at the whole question with an imaginative slyness worthy of Jorge Luis Borges. What, he asks, might have happened if the American Revolution had not happened? It takes a remarkable immersion in all aspects of an historical moment to play such hypothetical games, and a remarkable talent to carry the game through. The stakes are high, but the rewards are huge in that the method enables us to see what occurred as suddenly transmogrified by the illumination of what did not.

Brown earns the right to such flights of fancy by setting his more extravagant experiments in projection against a sustained ability to summarize and criticize the scholarly status quo. He is often at his best when slicing through the Gordian knots of scholarship that have twisted themselves around certain key questions. Take for example the endless debates over anti-slavery and the economic decline theory. Eric Williams was the first to argue at length that abolition occurred when it did in Britain because economically the British slave trade, and plantation slavery generally, were in mortal decline. Having astutely assessed the entire history of subsequent disputation between the pro- and contra-decline camps, Brown puts the whole question to bed with a footnote: "The case I make here for a new approach to the study of British abolitionism depends not at all on the existence of decline, or what decline means. With or without decline abolitionism in Britain still needs to be explained" (p. 16, n. 12). This statement reveals a morally centered and truly inquiring intelligence that can sidestep the solipsism of morally paralyzed economic historians.

Brown's book, as its title so eloquently attests, is concerned with more crucial questions than whether Atlantic slavery was or was not cutting a certain profit at a certain date. He reaches out beyond the issue of money (capital) to consider how class theory, revolutionary theory, and imperial theory interacted with and were embattled over the marketing of ideals (moral capital). Yet this bookis also a timely reminder of how mercurial and dangerous the inheritance of "moral capital" might be. In his suggestive conclusion Brown looks forward to the manner in which the British inheritance of anti-slavery was used to underpin later nineteenth-century imperial depredations in Africa. Once the British had invested in, indeed patented, anti-slavery as their own special brand of moral capital, then it could be appended to, or invested in, any activity. If British imperial activity in Africa was "anti-slavery," anything and everything could be justified around it. By making us look afresh at the moral landscape in which anti-slavery came alive Brown has opened up a field that has enormous implications for the study of the development of anti-slavery thought across the Atlantic diaspora after 1807. It is for this reason that his book will lie at the heart of subsequent debates over the moral engineering of abolition for decades to come.

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TIMOTHY LARSEN. *Crisis of Doubt: Honest Faith in Nineteenth-Century England*. New York: Oxford University Press. 2006. Pp. ix, 317. $110.00.

The main body of this book consists of a series of seven case studies of English popular freethinking and secularist leaders who, between the 1820s and the 1870s, eventually reconverted to Christianity. Timothy Larsen acknowledges that it is impossible to quantify the numerical extent of such reconversion, and his own methodology has thus been to explore prominent and well-documented cases rather than to attempt a systematic survey. He provides thoroughly researched accounts of the lives and thought of William Hone, Frederic Rowland Young, Thomas Cooper, John Henry Gordon, Joseph Barker, John Bagnall Bebbington, and George Sexton. An appendix provides briefer accounts of nearly thirty other individuals whose development, Larsen suggests, showed elements of a similar trajectory. Thus at one level this book is a valuable biographical resource for information on a series of fascinating men whose careers—crisscrossing the borderlands of faith and doubt, organized Christianity, secularist societies, and radical politics—deserve to be much better understood.

Around the case studies Larsen develops a wider argument that merits serious consideration by all historians of Victorian religion and culture. Reconversion, he shows convincingly, was a sufficiently widespread phenomenon to throw into serious question an overall narrative that characterizes the Victorian era merely with Matthew Arnold's "melancholy, long, withdrawing roar" of the sea of faith. He shows that secularist convictions quite often proved unstable and transient. Returns to Christianity occurred in active middle life, and cannot be dismissed as indicative of incipient senility or fear of imminent death, or as driven by a need for financial security. All Larsen's main examples were active Christians for significant periods after their reconversion, writing apologetic publications or even serving in the ministry, and none of them again reconverted to secularism. In view of the important leadership roles in the freethinking movement previously played by his case studies, he sees (p. 232) their reconversions as analogous to an espousal of secularism by several An-

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glican bishops. (In reality of course, no such apostasy ever occurred, and even the church's most famous episcopal "heretic," John William Colenso, emphatically sought to reform and redefine his faith rather than to renounce it.) Throughout the period, Larsen argues, Christian beliefs retained intellectual ascendancy.

This overall argument is a bracing one, and in seeking to convey it clearly and coherently Larsen can be forgiven for a degree of overstatement and combativeness. He rightly exposes the tendency of some scholars to assume rather than to demonstrate the decline of faith in mid and late Victorian England and too readily to treat "honest doubters" such as George Eliot and Arnold as normative rather than exceptional figures. He does not deny that there was a "crisis of faith" but maintains that this needs to be set alongside a parallel "crisis of doubt."

Inevitably Larsen raises as many questions as he answers. His case studies primarily reflect the culture of individualistic, autodidactic, plebeian Nonconformity. Can similar examples be found of more socially elite, Anglican, Oxbridge-educated individuals who reconverted to Christianity, or in such circles was loss of faith both less dramatic and more permanent? Are there any inferences to be drawn from the fact that all of Larsen's extended case studies, and all but two of his shorter examples in the appendix, were men? He does not engage with the question of whether such reconversions were primarily a gender-specific phenomenon, or whether his omission of female case studies arises from a lack of accessible evidence. Finally, were experiences of this kind distinctive to England, a country in which the boundaries between faith and doubt, secularism and Christianity were more permeable than in France, and operated very differently from those in the United States? Within the United Kingdom itself are there examples from Scotland, Ireland, and Wales—or indeed a lack thereof—that might help to advance understanding of such comparative questions? It is no negative criticism of this fascinating book to conclude that there is a great deal more still to be written about the "crisis of doubt."

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PETER MANDLER. *The English National Character: The History of an Idea from Edmund Burke to Tony Blair.* New Haven, Conn.: Yale University Press. 2006. Pp. x, 348. $35.00.

On or about 1848 "the idea of the English national character" changed, asserts Peter Mandler in his new book. Mandler's is clearly a history of "an idea," in the words of his subtitle. The bulk of his sources are works of history, treatises, speeches, sermons, national newspaper leader articles, travelogues, "auto-ethnographies," poll and surveys, and "national inquests at moments of crisis" (p. 5). In what seems to have started partially as a respectful argument with Linda Colley's *Britons: Forging the Nation, 1707–1837* (1992), among other works, Mandler traces the rise of discussions about Englishness to moments of uncertainty in domestic rather than international politics. The notion of national character, he argues, "developed for the first time into a serious and respectable category of self-analysis for the writing and reading classes" in the mid Victorian decades, in the wake of Chartism and Anti-Corn Law agitation in England and the 1848 nationalist revolutions on the continent (pp. 58–59). Following Stefan Collini, Mandler contends that there was a shift for the first time in the belief that the mass of Englishmen possessed "character." Citing material changes, such as urbanization, the railway, telegraphy and steam printing, along with the growth of social statistics and early social psychology, the nation became "a cultural unit" only in the 1850s. In Mandler's words, "the British were coming to see themselves as more homogeneous and uniform, less fissured by differences of region, religion, morals and class if not gender" (p. 66). (Historians of gender are likely to be repeatedly frustrated by statements such as this.) This was the period when Thomas Babington Macaulay, Henry Thomas Buckle, and William Stubbs wrote their histories of the English, and Charles Dickens penned his own characterizations in books like *Our Mutual Friend* (1864–1865). "Teutomania," with its emphasis on England's Anglo-Saxon roots and "ancient constitution," allowed English dominance of the union to be taken for granted. In Mandler's reckoning, there were 112 "Victorian textbooks on the history of Britain," 108 of which "called themselves histories of England" (p. 67). After the Reform Acts of 1867 and 1884, we learn, there was a revival of more conservative notions of patriotism "based on leadership, emulation and institutions" (pp. 106–107). The "Great Briton" of *Punch*'s John Bull personified for a time the English national character until the Boer War and World War I made such complacent self-imaginings impossible. John Bull was then replaced by the "Little Man," Sidney Strube's cartoon featured in the pages of the *Daily Express* from 1920 to 1947. The "Little Man's" representation of national character was a strange mélange, incorporating an older imagining of the English gentleman, along with the working-class "chirpy Cockney" and "the bourgeois suburbanite" with his pipe, his garden, and his nuclear family. Mandler convincingly argues that only in the 1950s and 1960s was the English national character recast in the image of the gentleman across the political spectrum. Depending on one's politics, though, the gentleman was the villain who had to be ousted or the gallant defender of traditional English values under threat in the postwar world. The death of the idea of national character, according to Mandler, came in the process of cultural change in the course of the 1960s (p. 221). In Mandler's reading, the concept "had lived too long" and "been stretched too far" "to function any longer in a credibly monolithic way" (p. 221).

Not only has Mandler given us, as we would expect, a carefully researched and thought-provoking work on

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the idea of English national character, he has also given us something else. Clearly, courting controversy, his book offers up a new challenge as well as a new set of insights into how to proceed in a scholarship that Mandler deems has too frequently relied on ahistorical assumptions about English or British "national identity." He argues that "some of the standard works" on national identity have claimed that there "is no tradition of writing about it" (p. 9). By changing terms to focus on "character" rather than "identity,"this book reveals a wealth of sources. Obviously at odds with identity-based approaches, Mandler has written a history of something that he contends is more complicated and that allows him to show that there are "multiple forms of national consciousness" that "interact, ebb and flow" (p. 6). This history is convincingly executed, especially up to the 1960s, although some scholars will be disappointed by the way the focus allows for a bracketing off of questions of race and gender. What we need after Mandler's book is a parallel history based on more demotic sources across this two-hundred-year period, which applies the same intellectual rigor to these texts that Mandler has applied to his.

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CAROLYN A. CONLEY. *Certain Other Countries: Homicide, Gender, and National Identity in Late Nineteenth-Century England, Ireland, Scotland, and Wales*. (History of Crime and Criminal Justice.) Columbus: Ohio State University Press. 2007. Pp. ix, 255. Cloth $49.95, CD $9.95.

In this unique and lucid study, Carolyn A. Conley examines perceptions of and responses to criminal homicides in England, Wales, Scotland, and Ireland between 1867 and 1892, a period that witnessed not only an overall decline in the number of homicides but also, in the context of the development of a more civil and humane society, increasing public concern with, and condemnation of, interpersonal violence. By reference to some 7,000 homicide reports and almost 6,000 homicide trials, gleaned from an impressive selection of primary sources, including a host of national and provincial newspapers, Conley demonstrates that although the nations of the United Kingdom shared a common parliament, they reacted to similar homicide cases in different ways and for different reasons, which reflected basic distinctions in national cultural values and assumptions.

Throughout the period, there were considerable variations in the verdicts of judges and juries in the United Kingdom, which reflected cultural diversity. A conviction for murder was four times more likely in England than in Scotland and Ireland, even though English juries returned for the (lesser) crime of manslaughter in almost half the convictions in homicide trials. The sentences imposed by the courts also varied. In England, fifty-eight percent of death sentences were carried out. In Ireland, although only four percent of homicide trials resulted in a conviction for murder, sixty-seven percent of those sentenced to death were executed, while in Scotland only thirty-four percent of death sentences resulted in execution. Sentences also varied in convictions for manslaughter, ranging from an average of three years' imprisonment in Ireland to five years in Scotland and six years in England and Wales. As Conley points out, the most significant elements influencing verdicts and sentencing patterns were social status, ethnicity, gender, age, familial ties, provocation, and the prior reputation of the killers and victims. Yet she also shows that respectable middle-class perceptions (which all-male courts and the press represented) of killers as foreign or psychotic, symptomatic of otherness, were misplaced, for most killers had no prior record of criminality and in most homicide trials the defendant was known to the victim within friendship and family circles.

The contrasting responses of the courts were also influenced by issues of national identity. In England, where respect for the law was deemed a key ingredient of the English character, those who failed to live up to this standard (especially those who belonged to a residuum perceived to be beyond civilizing influences) had to face the ultimately terrible consequences. In Wales, where there was a low level of interpersonal violence, juries often resolved homicide cases (which were presided over by English judges according to English law) within their own culture according to their own good judgment, which sometimes resulted in considerable variance between the judge's charge and the jury's (occasionally eccentric) verdict. Scotland, with its own judicial system, had a different concept of accountability, for while convicted killers were expected to atone for their sins, justice was often tempered with mercy. However, Scottish concepts of crime and punishment were also clearly influenced by ethnic issues arising from Irish immigration, which, it was held, threatened to pollute native society; hence mercy was rarely extended to those unworthy Irish "savages" who were convicted of murder. By contrast, in Ireland, where there was not only a longstanding tradition of, and sympathy for, recreational violence but also a distrust of courts that were perceived to be tainted by "British justice," juries were often inclined to see murder as the accidental product of the passionate nature of the Irish people, and they responded accordingly.

Contemporary responses were also influenced by issues of gender and family. As Conley observes, ideals of femininity were at variance with the reality, and, in dealing with females convicted of murder, including that of husbands and children, English juries often resolved this conundrum by differentiating between "respectable" ladies and lower-class women. By contrast, relatively few Welsh homicides involved female killers, and these women were generally treated with leniency by juries, two-thirds of them serving fewer than two years' imprisonment (although communal disapproval of their actions may have been a more severe punishment). Scottish courts held that women were just as

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prone to violence and accountable for their actions as men, although they often showed flexibility in dealing with the crime of infanticide, which was not automatically regarded as symptomatic of madness or deserving of capital punishment. Likewise, Irish courts barely distinguished between male and female killers but, since most Irish homicides normally resulted from brawls between men, Irish men who killed women (and who therefore deviated from the norm) were more likely to be found insane and treated accordingly, rather than being executed, unless the victim had also been raped.

By showing that although the courts of the United Kingdom variously regarded homicide as the result of carelessness, as an understandable reaction to provocation, as a violent act symbolizing an alien mentality, or as a threat to the existing social order, their perceptions of the seriousness of the offence and the punishments they prescribed were determined largely by what their own nation would tolerate, this splendid volume represents a major contribution to the historiography of Victorian crime and punishment. Moreover, the inclusion of Ireland (sadly neglected in most studies of Victorian crime) is, for this reviewer, particularly welcome. Conley is to be congratulated for her efforts.

ROGER SWIFT
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ROBERT SELF. *Neville Chamberlain: A Biography*. Burlington, Vt.: Ashgate. 2006. Pp. xi, 573. $59.95.

Since his death in November 1940, only two full-length biographies of Neville Chamberlain have been published prior to this one. The first, by Sir Keith Feiling, was published in 1946 and the second, by the Conservative politician Iain Macleod, was published in 1961. Thereafter the historian David Dilks was commissioned to write an official biography, the first volume of which appeared in 1984. This covered the years down to 1929, but the second volume, despite the passage of twenty-three years, has failed to materialize. The appearance of Robert Self's book is, therefore, a major event, for it is the first full-scale biography of Chamberlain to appear since all the relevant archive resources became available to researchers. (Self, therefore, has not faced the obstacles that were so spitefully put in the way of Feiling by the Cabinet Secretariat, and he has had full access to the records of the British government in addition to the Chamberlain papers and the papers of many of his contemporaries.)

It has to be said at the outset that Self has made full use of these sources and that the work is to be commended for its massive industry. Indeed the author has been working on Chamberlain for many years and, prior to the publication of this biography, had edited and published four volumes of Chamberlain's diaries and papers. But having said that, the work itself offers very little that is startlingly new about this controversial British prime minister, arguably the most reviled one in the twentieth century. That, moreover, is not the most basic weakness of this work. Its besetting fault is that it is very densely written and has the feel of a collection of seminar papers. In other words, it is not going to be very accessible to the general reader. A historical biography should communicate not just with the world of historians but also with the public at large. Admittedly, Chamberlain was a complex character, and the events he participated in and presided over were complex, but lucidity does not require oversimplification for its achievement. The contextualization, too, is very poor, particularly with regard to the chapters on the 1920s, during which Chamberlain made his reputation as a social reformer. Perhaps the best chapter in the book deals with Chamberlain's record as chancellor of the exchequer. Here the contextualization is excellent and the narrative crisp: the achievements of Chamberlain's classical approach to solving the problems of recession are sympathetically assessed against the Keynesian alternative and the judgment is balanced.

Given the wealth of research that has taken place on the foreign policy of the National Government since the introduction of the Thirty Year Rule, the reader might have expected the author to present some penetrating new insights into the foreign policy of the Chamberlain government, but one looks in vain for such analysis. Apart from the facile comment that appeasement had been the foreign policy of British governments from the middle of the nineteenth century, there is little indication in this book of the degree to which Chamberlain's policy toward Germany and Europe had been consensually formulated. Instead the image presented is one of an overbearing and dictatorial prime minister always bent on having his way. The truth of the matter is that the fundamental line that Chamberlain's government pursued was formulated during the first three months of 1936, well over a year prior to his becoming prime minister. While it is evident that as chancellor of the exchequer Chamberlain had a very substantial say in its crafting, the Foreign Office was fully complicit too, despite the fact that after 1940 many Foreign Office officials sought to distance themselves. One of the problems is that Self fails to address the issue of what the word "appeasement" meant to Chamberlain and his colleagues. He comes close to this at the beginning of the chapters on foreign policy, when he describes the aim of British policy as "pacification"; this is precisely what "appeasement" meant in the 1930s, but the use of the term thereafter indicated something quite different.

This work is not without errors. The author refers to the Special Areas Acts as the "descendants" of postwar regional policy (p. 231), where he should have written "ancestors." He suggests that the city of Salzburg could be seen from the picture window of Adolf Hitler's house on the Obersalzburg (p. 312). The view in fact was of the Untersberg mountain. Nevertheless, if dull, this is a worthy study.

ANDREW CROZIER
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RICHARD WIGG. *Churchill and Spain: The Survival of the Franco Regime, 1940–45*. (Routledge/Cañada Blanch

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Studies on Contemporary Spain.) New York: Routledge. 2005. Pp. 211. $150.00.

World War II dragged in even declared neutrals. The United States was neutral until attacked by Japan, yet the U. S. government aided Britain before that with various deals. Similarly, neutral Sweden, Portugal, and Spain, witnessing the fate of Belgium, the Netherlands, and Denmark, played both sides. Francisco Franco's equivocations could not mask his keen preference for the Axis, yet his vicious dictatorship survived until 1975. For this regrettable feat Franco exudes a powerful fascination, although historians mostly concur with Adolf Hitler's own advisers that Spain was a sideshow.

While Richard Wigg's first hook for another work on Franco's Spain is a title including Winston Churchill's name, he acknowledges that the prime minister's personal input on that topic, although important, was necessarily limited given the scale and global scope of his wartime role. Nevertheless, Wigg has assembled numerous primary sources, many inevitably well known but with some minor newly released archival material. Reinterpretation is welcome but requires better flagging of secondary sources. A more important question is whether Wigg's book sustains his second and main hook: the contention that Churchill's efforts to keep Franco to a degree of neutrality "ignored the other strand of Allied war policy, the fight for democratic values and thus helped prolong the regime until 1975."

Certainly Wilsonian ideals were woven into Franklin Delano Roosevelt's Atlantic Charter of 1941, yet neither the United States nor Britain entered the war to sustain democracy. Roosevelt's attitude toward the dictators after Pearl Harbor was vengeful. Once Hitler had attacked Poland there was no honorable or practical option for Neville Chamberlain but to declare war. Taking over in 1940, Churchill declared the first object of the war to be "victory, victory at all costs."

Churchill understood that Britain's key policy regarding Spain was to protect the gateways to the Mediterranean and the Suez Canal: to do that he had first to defeat the Wehrmacht in North Africa. The United States' entry into the war and Soviet victories at Stalingrad and Moscow signaled Hitler's eventual defeat but passed Allied command largely to the United States. However, geopolitical context remains distant in this account, which concentrates on the minutiae of the painstaking diplomacy of the British embassy in Madrid under Sir Samuel Hoare, who assiduously tried to nudge Franco away from his Axis pals (to whom Spain was in debt and Franco personally in thrall for refusing Hitler's demand for bases).

British and American policy required preemptive buying of essential Spanish stocks of strategic minerals otherwise available to Germany, which could absorb unlimited quantities of wolfram, zinc, and other minerals into Hitler's munitions program. That Churchill's disagreement with Roosevelt's tough stand on Spain over oil exports and wolfram imports loomed large at times forms the basis of Wigg's argument that Churchill, understanding Spain's postwar strategic and economic importance to Britain, soft-pedaled when he could have followed Roosevelt's hard-line policy toward Franco in 1944–1945. Here contextual statistics would be helpful. Churchill knew that the closing stages of the war would be the most vicious, and that postwar Europe would require continuous effort for years to come. He spoke "kindly words" to remind the House that Franco could have done much damage to the Allied cause. As he explained to Roosevelt, "I do not care about Franco, but I do not wish to have the Iberian Peninsula hostile to the British after the war."

Ultimately Wigg accepts this, concluding that Churchill was "indeed a great war leader but he was no friend of Spain." Franco lived on for thirty years, imposing misery and isolation on Spain, which remained an international pariah, excluded from the United Nations and then from NATO. As Churchill knew, nonintervention in the Spanish Civil War had not worked. But sanctions, too, are a form of intervention, and as the Allies discovered in Russia in 1919, and as we see plainly in our own time, these can be treacherous tools.

JILL EDWARDS
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RONALD HYAM. *Britain's Declining Empire: The Road to Decolonisation, 1918–1968*. New York: Cambridge University Press. 2006. Pp. xvii, 464. Cloth $85.00, paper $32.99.

The last twenty years or so has seen the steady increase of historical publications devoted to the twentieth-century decline and decolonization of Britain's colonial empire. The latest in a succession of surveys, which includes most notably those by Robert Holland, John Darwin, and Bernard Porter, is Ronald Hyam's new book. Aficionados of Hyam's work will detect in it all the qualities which made his earlier study, *Britain's Imperial Century, 1815–1914: A Study of Empire and Expansion* (3d ed., 2002) so deservedly admired and widely used. Readers will welcome its comprehensive range of secondary reading, its mastery of a daunting body of primary archival sources, and its marriage (no shotgun wedding here!) of encyclopedic reference with wide-ranging argument.

However, this is no simple sequel to the earlier study. Hyam spells out succinctly the differences: "it is . . . focused more upon a single theme, 'the end of empire' in its political aspects," and "is more closely based on archival research" (p. xi). The latter characteristic has been made possible by the monumental labors over almost forty years of those scholarly editors (of whom Hyam is one) engaged in the British Documents on the End of Empire Project, and in earlier similar series on India and Burma. These multivolume projects have had as their goal the charting of the end of empire as revealed in the British official records preserved in the National Archives at Kew, London. Hyam also positions himself unequivocally in terms of the historiography of decolonization. "Historians have offered four

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main options for explaining the end of empire" (p. xiii): it was brought down by nationalist pressure; it was overcome by economic weaknesses; its rulers experienced a collapse of confidence in the face of competing domestic demands; it succumbed to international criticism channeled chiefly through the United Nations. Avoiding the easiest option of judicious even-handedness, Hyam argues for both "the conclusion that the international dimension was the most important of all" (p. xiv), and a terminus in 1968.

So far, this has been perhaps the explanatory option least popular with historians of decolonization. Nevertheless, Hyam argues his case with considerable verve and persuasiveness. Taking as his starting point the evident dysfunctionality of the interwar empire (illustrated, for instance, in the failings of Indirect Rule and white settler colonialism), he charts the steady growth of "pragmatic responses" to the kaleidoscopic variety of local colonial circumstances. This "most visible aspect" of colonial management manifested itself in the outlook and reactions of government ministers and officials as "a willingness to decolonise." After 1945 a "culture of decolonisation" took hold as policy makers fell back on withdrawal as the only plausible manner of maintaining what mattered above all if descents into material bankruptcy, peripheral chaos, international derision and instability, and a third world war were to be avoided: that is, Britain's international credibility and prestige. Insofar as decolonization helped to achieve at least some of these things it represented a success for "the official mind," unhampered in its pursuit of essentials by the demands of special pressure groups, the media, and business. Still more irrelevant to the unravelling of empire was any amorphous "public opinion" or popular culture; empire had never generated much commitment among the wider population at home.

Hyam is a master of both the brief biographical sketch and the succinct summary, skills brought together in his handling of central episodes in his tale. They enliven his account immeasurably and reveal a rich vocabulary of characterization. His terse account of the Suez Crisis takes issue with earlier analyses, mounting a powerful attack on Anthony Eden's aberrant behavior and the suspension of normal government processes. In dealing with Harold Macmillan's "wind of change" speech, Hyam very cleverly weaves together his dissection of policy discussions with the contribution of individuals to their shape and direction. No more than the "ineffably awful Harrovian" do fellow historians escape the consequences of Hyam's forensic archival work: "I find it hard to recommend this book" (p. 230).

It is difficult to be sure where this sharply perceptive study leaves the history of Britain's declining empire. Surely it will provoke vigorous reactions from those with rather different perspectives. No less certainly it offers argument unlikely to be surpassed for a long time in its command of the archival record. It is both textbook and much more than that, appealing to students by its engagement with debate and its offer of much-needed guidance through a world of "variety," "strangeness," and "complexity," and to professional historians for its range, originality, and stimulus. This is a moment when one might have expected a lull in the expansion of the subject's historiography. Hyam's book will make any such hiatus highly unlikely.

ANDREW PORTER
King's College London

K. D. M. SNELL. *Parish and Belonging: Community, Identity and Welfare in England and Wales, 1700–1950*. New York: Cambridge University Press. 2006. Pp. xiv, 541. $110.00.

There was a time when the study of the parish was regarded as the essence of antiquarianism among academic historians of the British Isles. But, for over two decades, the work of K. D. M. Snell has done a great deal to remind us that sophisticated and close studies of the parish, with its rich and abundant records, are essential to a fuller understanding of British experience in former times. In many respects, one can regard this new book as a lengthy sequel to Snell's *Annals of the Labouring Poor: Social Change and Agrarian England, 1660–1900* (1985), which was similarly concerned with the ways in which social relations, family formation, occupation, and mechanisms of poor relief were all highly localized. The more recent book takes the subject well into the twentieth century whereas the earlier work treats the so-called long eighteenth century with more depth and originality (particularly in its use of primary evidence from that earlier period). It is on the nineteenth century (where these two books overlap) that Snell's new study is most energetic and most impressively supported by his application of a wide range of period evidence to quantitative and qualitative analyses. The most illuminating findings support his argument that the decline of community (a vexing issue in a centralizing and globalizing society) was not produced by nineteenth-century industrialization but by a much later, gradual decline of the parish itself as the real and perceived basis of community in England and Wales.

As Snell shows, throughout the period considered in his book the parish persisted as a vital agent in both the practical administration of local communities and the formation of collective identities. Snell properly recognizes that in each of these functions the parish, with regional variability, subsumed a range of community institutions including, for example, some smaller townships in northern England. That the term parish was used not only in its ecclesiastical sense but more broadly to refer to the principal community is, perhaps, most revealingly demonstrated by Snell's consideration of Welsh parishes where Anglican conformity was the exception rather than the rule. In a more general sense, the book succeeds in showing that, well into the twentieth century at least, the administration of the practical needs of a localized community constituted a form of culture by which more highly subjective notions of al-

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legiance, affinity, and belonging were produced, replicated, communicated, and regulated. This normative parish culture defined communal experience in England and Wales in demonstrable ways (high degrees of endogamy, for example) and to a profound degree despite the rise of national institutions and the bases of new collective identities (working-class consciousness, for example) on a national scale.

This book makes such a powerful and welcome contribution that it seems almost churlish to raise objections. Nevertheless, it is fair to question Snell's use of the eighteenth century as part of the subject matter of this book. The objection is not that the book is unsound in its treatment of the earlier period; rather, it could have established continuity between the early modern period and the modern period (the focus of Snell's archival research effort) with a much more succinct introductory nod to the existing scholarship (including his own) on the seventeenth and eighteenth centuries. Then the book could have been devoted to Snell's fresh analyses of the nineteenth and twentieth centuries. As it is, those chapters with portions delving into the earlier period are either conspicuously less original than chapters resting squarely on later evidence or they neglect to engage important arguments. So, for example, one could simply read Tim Hitchcock (on eighteenth-century urban poverty and poor relief), Steve Hindle (on early modern rural poverty and poor relief), and Carl Estabrook (on early modern parochial identity formation and xenophobia). And, one could wish for more consideration of Mary Fissell's work (on early modern corporate alternatives to parish poor relief) or the work of Jonathan Barry and Kenneth Morgan (on the parochialism of eighteenth-century Anglican devotional practice). The book does include a fascinating chapter on the expression of local attachments in gravestones, evidence resourcefully mined by Snell. But, it is surprising that such a carefully considered book, with wide-reaching implications, largely relegates to the periphery issues of religious culture arguably essential to the history of English and Welsh communal identities in any period. There can be no doubt, however, that this book is a major scholarly achievement. It has advanced our understanding of the parish's essential influence by demonstrating the persistence of that influence well beyond the early modern period and, possibly, into the present.

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ANDREW CHANDLER. *The Church of England in the Twentieth Century: The Church Commissioners and the Politics of Reform, 1948–1998*. Rochester, N.Y.: Boydell Press. 2006. Pp. xii, 542. $65.00.

Andrew Chandler's history of the Church Commissioners is neatly circumscribed by their foundation in 1948 and the major changes to the institutional structures of the Church of England in 1998. It is, to all intents and purposes, an official history, and evidences both the strengths and weaknesses that flow from that. On the one hand, Chandler was afforded access to a cornucopia of archival material hitherto almost untouched by historians. This enables him to detail a vital but unduly neglected aspect of church history: the management of the church's assets and the disbursement of expenditure—through the payment of stipends and related benefits such as pensions and accommodation to clergy, and the building, maintenance, and disposal of the often ancient building stock in its care—in pursuit of its continuing responsibility for the cure of souls. It is to his credit that, despite the dense technicality of some passages, the book remains readable, although more tabulation of financial details would not have gone amiss. For, on the other hand, this is a very long book. Despite some good case studies, the mass of information resulting from Chandler's unprecedented access can obscure more than it illuminates. Contextualization is sometimes poor: only those familiar with the Anglican-Methodist ecumenical conversations 1955–1972 would follow the scattered references here. There are simple errors, such as the name of the Missouri college that shipped across St. Mary Aldermanbury. And we are told more about the Commissioners' reactions to developments such as the 1964 Paul Report on the deployment and pay of the clergy than those developments themselves. Chandler certainly shows awareness of the enormous strains placed on the church's deployment of resources by inflationary pressures and demographic change. He seems far less conscious of the way in which Robin Gill's *The Myth of the Empty Church* (1993) provides a model for measuring the reactions to those strains, or of Gill's influence upon institutional reform in the church in the 1990s. This is very firmly, and sometimes narrowly, an institutional biography of the operational side of the Church of England.

Despite its centrality, however, the commission seems to have been misunderstood or neglected, even by the archbishops supposedly directing it. Of the five primates covered here only Geoffrey Fisher (archbishop of Canterbury 1945–1961), who could count himself among the founding fathers, showed sustained interest in its activities. This makes Chandler's decision to organize the book chronologically through the primacies of Fisher and his successors seem odd.

It was under Fisher that the historic institutions of Queen Anne's Bounty, for the maintenance of poor clergy, and the Ecclesiastical Commissioners, for the management of the established church's estates and revenues, were amalgamated in 1948. Chandler persuasively draws parallels with the Attlee government's contemporary nationalized corporations, often similarly charged with the rationalization of ramshackle organisations. The challenges were many: an unequally rewarded clergy with, in contrast to many of their Nonconformist brethren, almost no pension arrangements; the maintenance of a universal parochial structure for an increasingly unchurched nation; a diocesan organization that frequently contested the Commissioners' right to manage resources; often hard to maintain

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buildings, some in the wrong locations; and, by no means least, the need to increase funds if all these challenges were to be adequately addressed. If anything, Chandler's reliance on the Commissioners' internal documents results in an underplaying of their successes in meeting these problems and an obscuring of whether it is the benign economic climate of the 1950s and 1960s or astute management by Fisher and Sir Malcolm Trustam Eve that accounts for steady success in these years. Similarly, the presentation of the reordering of the church's estate underplays the dubious benefits of ecclesiastical exemption from restrictions on listed buildings and too often relies overmuch on the view from the Church Commissioners' offices of the increasingly vociferous architectural heritage campaigns of the 1960s onward.

Progress on the liabilities side, such as the Pensions Measure (1961) or increases in clerical stipends, however, had to be matched by performance on the income side, either by shedding surplus buildings or by increasing investment yields. Pressure to achieve the last, faced with the higher inflation of the 1970s and 1980s, led the Commissioners to develop those latter-day cathedrals of material culture, shopping malls such as Gateshead's MetroCentre. A combination of overspeculation and recession hitting its U. K. and U. S. investments, however, led to a cash flow crisis in the early 1990s and, in 1998, to the imposition of new structures of accountability upon the Commissioners. Chandler tells this story punctiliously, although with overscrupulous unwillingness to apportion blame. Being official, this work tends to concentrate on what the Commissioners did, rather than assessing the outcomes, or the impact of all this activity on the cure of English souls.

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ANDREW R. HOLMES. *The Shaping of Ulster Presbyterian Belief and Practice, 1770–1840*. New York: Oxford University Press. 2006. Pp. xvi, 374. $99.00.

In the last thirty years there has been quite a revolution in Irish ecclesiastical historiography. It started with the revisionist approach to Irish Roman Catholic history in the work of Sean J. Connolly, Patrick J. Corish, Donal A. Kerr, and others, and more recently has included some excellent new studies of Irish Presbyterianism—though unfortunately, as yet, very little new on the history of the pre-disestablishment Church of Ireland. Andrew R. Holmes's book is an excellent contribution to this revisionist historiography and has clearly been influenced by Connolly's approach to Irish Roman Catholic history in *Priests and People in Pre-Famine Ireland, 1780–1845* (1982). Holmes's concerns are not primarily with the political implications of religious association but with the internal structures and disciplines of Irish Presbyterianism in the late eighteenth and early nineteenth centuries, which he rightly sees as the formative period in modern Irish religious history, the one in which the sectarian divides that bedevilled Ireland for the next 150 years, and which are only just beginning to be resolved, began to be developed. Crucial to this development was the, comparatively late, Evangelicalization of the main Presbyterian group in Ireland, the Synod of Ulster, which took place largely through the efforts of Henry Cooke.

The main purpose of Holmes's book is to look in depth at how this process of Evangelicalization occurred, by examining the theological beliefs and liturgical practices of the Presbyterian community in Ulster. A long introduction covers the previous historiography of the topic, the economic and social background, and an overview of the Evangelicalization process. The core of the book is organized into four separate sections. The first concerns the private spirituality of the Presbyterian community: the observance of the Sabbath, the setting aside of special days for fasting and thanksgiving, and the residual attachment to the pre-Reformation church calendar. Officially this was disregarded by churches in the Reformed tradition but Holmes confirms that, as in Scotland, there was a good deal of unofficial observance of certain traditional festivals, such as Halloween, and an attachment to superstitious practices that the leadership of the churches found difficult to eradicate. The second section of the book deals with public worship, the use of psalmody, preaching, the sacrament of the Lord's Supper and the practice of church discipline that determined whether or not the laity were fit to partake of the sacrament. Again, as in Scotland, Holmes notes a determined attempt to reform psalmody in Ulster in the first half of the nineteenth century. The observance of the communion seasons followed the traditional Scottish practice and, as in Scotland, some attempts were made in Ulster, in the 1820s and 1830s, to increase the number of communion services. The third section of the book deals with "rites of passage"—baptism, marriage and burial—and the final section with what might be termed the "educational dimension" of religion: catechizing, religious education, Bible reading and informal meetings for worship. As was the case with both Anglicans and Roman Catholics in Ireland, Holmes confirms the great concern of many ministers in the 1820s and 1830s that the traditional methods of catechizing and visiting were less popular than they had been, and that ways had to be found to insure that members of the Presbyterian community were being properly educated in the essential rudiments of doctrine and religious practice.

In a sense this book does not tell the expert in the religious history of the late eighteenth and early nineteenth centuries anything that he or she did not know or had not at least suspected to be the case. Work across Europe looking at this period of religious history is confirming that it was, overall, a period of underestimated reform and one in which, irrespective of country or religious allegiance, very similar methods of religious reform were being adopted. In Ireland there was the particular dimension of Evangelicalism, much later in its impact than in England and Wales, although roughly

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contemporary with the Evangelical movement in Scotland, which was to transform not just Irish Presbyterianism but the Church of Ireland as well, and was to be a major factor in the increasing sectarianism of the period after about 1820. Holmes might have said more about this aspect of religious change in Ireland and explored further the extent to which Evangelical Presbyterians and Evangelical Anglicans were prepared to make common cause, for example in the "Second Reformation" campaign of proselytism among the Roman Catholic community in Ireland. However, in every other respect, this book is an exceptionally valuable contribution to the religious history of both Ireland and Europe, firmly set within the revisionist agenda of contemporary religious historians, and will be required reading for anyone who wants to know more about the development of Irish Presbyterianism in this crucial period of Irish history.

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AMBROSE MACAULAY. *Patrick McAlister, Bishop of Down and Connor, 1886–95*. Portland, Oreg.: Four Courts Press. 2006. Pp. 182. $40.00.

This is an account of the episcopate of Patrick McAlister of Down and Connor from 1886 to 1895. Although it lasted only nine years, it ran parallel to a very turbulent period of history associated with reaction to the Home Rule Bills, the riots they provoked, and the Parnell split. When McAlister was ordained bishop of Down and Connor in March 1886, he automatically stepped into a political role. In 1884 the Irish Catholic bishops agreed to support Charles Stewart Parnell and the Irish Parliamentary Party in its efforts to gain self-government for Ireland and reform of the landlord system, in return for the party's support for the bishops' goal of securing education rights for Irish Catholics.

This book presents a micro-study of what was taking place in Irish history in the 1880s and 1890s, but with the added dimension of sectarian tension in McAlister's diocese. By the time Prime Minister William Ewart Gladstone introduced his Home Rule Bill on April 9, 1886, hostility toward it was already manifest in clashes between Protestants and Catholics. Although the bill was defeated in June, rioting continued. Many people lost their lives, and there was much damage to property. A commission of inquiry into the riots concluded that the possible introduction of Home Rule was a key factor leading to the rioting. Both the report and the *Northern Whig*, the organ of liberal Unionism, praised McAlister and the Catholic clergy for the restraining influence they exercised on their people. While street violence was consuming Belfast, agrarian agitation was taking place in rural areas. McAlister had strong sympathy for the struggle of tenants against the landlords.

The Plan of Campaign instituted by the National League in response to the agrarian trouble caused disquiet in Rome, and this led to the visit by papal delegate Archbishop Persico in 1887 and subsequently to the 1888 papal decree condemning the Plan of Campaign. 1888 was a tense year for the bishops in terms of their relationship with Rome. For practical purposes, they ignored the papal decree. For his part McAlister wrote to Rome explaining the difficulties of the Irish situation and urging the pope to condemn the treatment by landlords of their tenants.

At the time of the Parnell split, McAlister followed the line of his fellow bishops and was vitriolic in his criticism of Parnell. No longer assured of the support of the Catholic-nationalist *Morning News*, McAlister decided to establish a new paper, and thus the *Irish News* was launched in 1891 to represent the views of nationalists and Catholics. With the prospect of the reelection of Gladstone and a second Home Rule Bill, tempers flared again; by now even liberal Protestants were vehemently opposed.

Macaulay gives a sense of the fraught political climate that the bishop had to negotiate. In order to discharge his duty to his flock, he had to deal with national and local government as well as Catholic, Church of Ireland, and Presbyterian vested interests—and all this against the background of Rome. A comprehensive range of primary sources is utilized, including data from diocesan, Vatican, and *Propaganda Fide* archives as well as sources from newspapers with both Unionist and Nationalist leanings. Chapters on education and pastoral developments and two appendixes provide a wealth of statistical information on the population of parishes and schools and church and school building programs.

A good biographer must impartially present the evidence, warts and all, which allows the reader to draw conclusions, and Macaulay is adept at this. We admire McAlister's resolute commitment to the care and interests of his flock in terms of their spiritual and temporal welfare, and yet we see that this same determination led to him being less than fair toward the Passionist community in his diocese. Sociopolitical developments in the nineteenth century had made the Irish bishops very powerful, and McAlister, to preserve his own power base, placed severe restrictions on the Passionists' ministry, to the detriment of the pastoral care of his people.

Here we see how the Passionists were caught in a web of forces not of their own making and over which they had little control. The Gallican reaction of the Irish bishops to the papal decree condemning the Plan of Campaign meant that relations between Rome and the bishops were already strained, and the Roman authorities were anxious not to provoke matters further. The timing of the conflict that ensued was unfortunate for the Passionists. While they had a case, such was the power of the local church that Rome was only able to prevail when McAlister died and it came to appointing his successor. Perhaps most valuable of all is the insight the book gives into the deep-rooted resistance to na-

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tionalist aspirations that existed in Belfast and the surrounding area long before the turn of the century.

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EVI GKOTZARIDIS. *Trials of Irish History: Genesis and Evolution of a Reappraisal, 1938–2000.* (Routledge Studies in Modern European History, number 7.) New York: Routledge. 2006. Pp. xi, 324. $130.00.

Evi Gkotzaridis has attempted the praiseworthy task of placing the development of Irish historiography in a general European context. If the author is not always successful—the tome sometimes suffers from overelaboration, repetition, and elongated explanations of the obvious—the effort yields some interesting results and justifiably can be hailed as the first full-scale attempt to pursue the trail of Irish historiography. The beginning date, 1938, is when T. W. Moody and R. Dudley Edwards, both products of the Institute of Historical Research in the University of London, founded *Irish Historical Studies* with the deliberate purpose of bringing standards of impeccable scholarship and objectivity to the study of Ireland's past. Gkotzaridis alleges that prior to 1938 it was impossible to write about Irish history without being embroiled in the sterile debate over England's wrongdoings in Ireland. Moody, Edwards, and their ilk succeeded to such a degree that this new methodology dominated Irish scholarship from the 1960s, and in the 1980s it provoked a reaction, led by Brendan Bradshaw, against what was termed "revisionism." "Revisionism," Gkotzaridis maintains, became a word of defamation against those who spoke with casualness and disrespect of the national heroes. Northern Ireland unionists adopted a similar stance, though obviously in reverse. Indeed, it is a limitation of the book that so little attention is given to unionist and notably Ulster unionist historiography and the popular reaction to these currents.

It was Roy Foster who more than two decades ago decreed that we are all revisionists now. Foster's much misrepresented remark meant more simply that contemporary historiography was devoted to recounting history in a way consistent with the wealth of documents becoming available. Foster did not imply that revisionism was a calculated formula for dismantling either nationalist or unionist patriotism. Gkotzaridis surely overstates the case for victimization. What anti-revisionists decried was the dominance of the new school in Irish universities and among the governing elite of the country; anti-revisionists believed it was they, not "revisionists," who were the victims of a new and uncomfortable historical orthodoxy. As the author observes, this new history was reflective of a wider Irish society that itself moved from parochialism toward a balance between regionalism and European cosmopolitanism. Whoever the "victims" were is a moot point; but either way, the answer reveals the interaction of historians and society. Gkotzaridis, though, makes the acute point that "revisionism" shifted the ground from oppression and exploitation for the benefit of a colonial power and prioritized a more troubling and embarrassing theme: the repetition of oppressive patterns in the name of the republican revolution. In this one is reminded of George Orwell's *Animal Farm* (1945). Absent in the account is the persistence of the older, essentially patriotic approach to Irish history among the considerable cohort of American academics in the field. Presumably, this is a group of historians that by and large has not found itself in need of responding to shifts in societal attitudes in Ireland and Europe. In a like vein, the author assigns too little attention to the impact of historical training and intellectual trends in British universities; these have had a profound influence on Irish historical writing both directly and indirectly.

It is more appropriate to emphasize the strengths of Gkotzaridis's account. The author does a fine job unfolding the controversy about how history should be written, for whom it is written, and how Irish historiography followed patterns of historical scholarship across Western Europe. Gkotzaridis usefully discusses the intellectual mood of the 1990s, revisionists as a new type of intellectual, the obstacles faced by historians, the epistemological and philosophical position of Irish revisionism, and similar issues. He illustrates his argument through writings on the great famine in a chapter subtitled "the crisis of representation and the limits of empirical history." For the author, the famine is perhaps best conceptualized as the site of a conflict between intentionalists and structuralists, for no other event of Irish history raises such sensitive questions of coherence and intention. The Irish revolution also is scrutinized and the impact of the *Annales* School emphasized. Perhaps the most original sections of the book deal with the underpinnings of historical writing and the ongoing problem of establishing objectivity. These, of course, are not issues exclusive to those engaged in Irish history; they have a general application. As an extended and penetrating essay on Irish history and historical writing more generally, this book warrants careful reading.

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CHRISTOPHER STORRS. *The Resilience of the Spanish Monarchy, 1665–1700.* New York: Oxford University Press. 2006. Pp. xii, 271. £60.00.

The "Decline of Spain" is perhaps the most contentious issue in the historiography of that country. From its pinnacle as the dominant power in Europe in the sixteenth century, the traditional argument goes, the Spanish monarchy or empire—a loose collection of countries that encircled the globe—declined precipitously, because of the failures of Philip II's heirs on the throne, until it reached its nadir at the opening of the eighteenth century, when it was largely dismembered. Recent years have seen a spate of works that attempt to

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provide a more nuanced view of the reigns of Spain's seventeenth-century monarchs Philip III, Philip IV, and Carlos II, with the latter, whose reign coincided with that of Louis XIV of France, being by far the least studied of the three. To fill that gap Christopher Storrs offers this book.

Storrs eschews a "revisionist" approach that would seek to overturn the traditional assessment of the reign—as one led by a decrepit monarch often described as an imbecile and insane, and one that witnessed the ultimate decadence and degradation of Spain militarily, economically, and politically—with a more positive view that would exaggerate the progressive elements of the reign at the expense of its clear difficulties. Instead he argues more modestly that, given the inherent difficulties faced by the monarchy in the period, we should at least give more credit to Carlos II especially, and his government generally, for "conserving" that empire essentially intact for his successor. Part of this approach requires that we understand that while Spain was no longer a hegemonic power, it was nevertheless still a significant actor on the global political stage, and one that could, despite its limitations, command resources sufficient to contend in the political arena on a relatively equal footing with other European powers (similar perhaps to the position of Russia after the fall of the Soviet Union). In chapters on Spain's armies, navies, finances, government, and the relationship between the center and the periphery of the empire, Storrs details just what those resources were and how the Spanish court managed to mobilize them to achieve its objectives in both war and peace.

As Storrs points out, while its resources were substantially reduced from their heyday in the reign of Philip II and its power in Europe suffered as a result, the Spanish monarchy nevertheless commanded the substantial armies, navies, and finances essential to its survival. The armies of Flanders and Lombardy, for example, were still the largest permanent armies in their regions, and Spain's navies were often the largest afloat in some areas, all capable of providing a considerable brake on the imperial designs of Louis XIV, either alone or in conjunction with those of other European powers. In each of the areas he considers, Storrs argues that the king was not only able to utilize traditional institutions and practices to maximize resources, but also that he introduced notable reforms and innovations, especially in the area of revenue, finance, and power sharing, which would underlay the more absolutist reforms designed to increase the authority of the Bourbon kings of the next century.

Storrs's work is clearly written and well organized, with an unambiguous thesis and conclusion (not only for the book, but for each chapter), and is therefore an academic's dream. But it provides a macro-level view. We get only the barest hint here of the actors involved or how their personal predilections could influence the working of policy in each of the areas outlined. For instance, while Storrs argues that the traditional image of Carlos II as an ineffectual leader is distorted, we get no direct evidence to the contrary, either from the king's own writings or from opinions of contemporaries. Quotation from sources is minimal, and we are often asked instead to trust that the obscure document referenced in the footnotes backs the point Storrs just made. This is particularly problematic when it comes to supporting his central contention that the king and his ministers were determined "to maintain the Monarchy," that this was "a priority of his government," and that the king himself was actively involved in setting the "conservative" agenda (e.g., pp. 60, 129, 165, 166, 190, 233). What we get is circumstantial evidence: the empire's resources and territories were conserved; therefore that was the intention. We need a more comprehensive view of the reign's "grand strategy" to assess whether this was indeed the case. To be fair, this book is a mere sketch of the reign, providing historians with a summary of details from a mass of largely untapped and uncataloged sources, and it therefore raises as many questions as it answers. Further work on the period is badly needed; Storrs points the way.

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JEAN-MARIE LE GALL. *Le mythe de Saint Denis: Entre renaissance et révolution*. (Époques.) Seyssel: Champ Vallon. 2007. Pp. 536. €32.00.

In his hilarious short story, "Refuge of Insulted Saints," Robertson Davies recounts the problems created when all the saints newly desanctified at Vatican II take up residence in a small, backwater Canadian college. Jean-Marie Le Gall's sprawling, important book on the myth of Saint Denis offers a decidedly more scholarly (if less funny) exploration of the fate similarly suffered by a figure once central to France's cultural identity. Le Gall traces the polymorphous meanings invested in Saint Denis to tease out significant changes in elite culture and modes of representing the French kingdom. While he makes occasional allusions to antiquity and the medieval era, Le Gall focuses on the early modern period to relate how and why the multifaceted myth of Saint Denis as a person and place progressively diminished in importance. This very complicated book seeks to establish an overall thesis of linear change by way of a non-linear if not elliptical narrative. The description is most thick, the analysis often quite subtle; a close, careful reading in the end proves well worth the effort, however. This review will focus on the general structure of the book and main conclusions that Le Gall draws, leaving to its eventual readers their own judgments about particular aspects of the kaleidoscopic tableau he creates.

The book is divided into three parts. Part one examines the legendary heritage of Saint Denis to elucidate the social, political, and religious factors that fused together his identity as the ancient Greek philosopher known as the "Areopagite," the first bishop of Paris, the protector of France's kings, and the apostle of the Gauls. The mystical writings in Greek relating his neg-

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ative theology—defining God by what "he" is not—found great appeal as interest in Neoplatonism grew in the Renaissance. The translation of this corpus of texts into Latin and then vernacular languages proved popular among humanists and theologians alike, who hoped to recover more fully the philosophical legacy of Hellenic antiquity. Despite being the legendary founder of the church in Paris, Saint Denis never enjoyed the kind of widespread veneration that might be expected, in large part, as Le Gall explains, because he served more identifiably as the protector of France's kings. Yet even in this capacity, Saint Denis's importance diminished in royal ceremonial and panegyric over time as new rituals celebrating absolute kingship developed, particularly under the Bourbons. The final key aspect of the myth of Saint Denis was his purported role as apostle to the Gauls. According to legend, this boon companion of Saint Paul brought Christianity to ancient Gaul before it even became established in Rome, thus giving the Gallican Church historical precedence. Yet the various myths surrounding Saint Denis proved highly unstable as time wore on.

In part two, Le Gall takes a closer look at the various controversies surrounding Saint Denis, all of which reflected important ideological shifts in French elite culture. Skepticism about Saint Denis's authorship of the Areopagite corpus and his role as founder of the bishopric of Paris informed the confessional polemics between Catholics and Protestants, bishops and members of the regular clergy, and partisans of mystical theology and those favoring scholasticism. These admittedly arcane controversies carried, as Le Gall demonstrates, keen significance on larger questions related to more critical forms of historical memory and tradition as well as the explosive relationship among science, religion, and politics. New methods of source criticism from humanists and Protestant reformers raised serious doubts about the historicity of Saint Denis's writings. Catholic apologists nonetheless continued to invoke him, though with decidedly less enthusiasm after 1650. Debates among Catholic clergy over ecclesiology and, thus, leadership of the visible church saw bishops and regulars alike invoke the saint for their own contrary purposes. As a result, the monarchy gradually abandoned its long affiliation with a saint whose legendary attributes encouraged behavior contrary to its interests. Saint Denis's credibility eroded in the eighteenth century and, despite efforts by *philosophes* such as Voltaire to invoke him as a national icon, Saint Denis eventually became the Pseudo-Dionysius we know today.

Part three delves into the changes affecting the place and practices at the royal abbey of Saint Denis and, to a lesser extent, the royal abbey of Montmartre. Both sites supported and thus helped defend, however vainly, the special forms of Dionysian devotion and the cult of royal religion associated with the sanctuary at Saint Denis as it became a necropolis for the Valois and Bourbon dynasties. The abbey of Saint Denis attracted the foundation of religious houses in its vicinity, all of which attracted a steady stream of clergy and pilgrims. Le Gall explores the evolving cultic practices there by focusing on the renowned "treasure" of Saint Denis. These "*objets de la mémoire*," made up of reliquaries, bejeweled tomes, and royal paraphernalia, possessed enormous symbolic and financial value and served as the main draw for visitors. Le Gall further examines the experience of going to Saint Denis by analyzing its special liturgies and the royal monuments built in this sanctuary, and to a lesser extent at Montmartre. The eventual rupture between the monarchy and the nation in the French Revolution, however, prevented the site from becoming a national "*lieu de mémoire*," to use Pierre Nora's notion. As a result, in the modern era Saint Denis became reduced to the odd, slightly eerie figure known mainly for holding his severed head, while as a place it became a tourist destination for its religious sites and a hardscrabble suburb for the working, now largely immigrant poor. Le Gall's rich study recounts the compelling story of this complex loss, from which readers have much to gain.

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MICHAEL WINTROUB. *A Savage Mirror: Power, Identity, and Knowledge in Early Modern France*. Stanford, Calif.: Stanford University Press. 2006. Pp. xii, 306. $60.00.

The eight chapters of Michael Wintroub's book orbit around a central event: the entry of Henri II into Rouen in 1550. A striking feature of this entry was the inclusion of a *tableau vivant* of Brazil, or what Henri's cosmographer André Thevet liked to call "*la France antarctique*." According to the book's introduction, in considering the royal entry from several different angles, the author aims to illuminate the multiple and often contradictory dimensions of urban elite identity in Rouen at the time, in particular in relation to the "cultural category of the exotic" (p. 6). Rather than a static portrait whose aim is to unify (and thus to reduce) its subject, this stimulating and innovative work attempts to do justice to the polyvalence and dynamism of a particular social group at a given historical moment.

After two chapters that sketch Henri's entry and describe its commercial and diplomatic backdrop, Wintroub examines the relatively neglected "barbarian" dimension of French identity in the sixteenth century as one of the contexts informing the potential functions and meanings of the Brazilian *tableau vivant*. The subsequent discussion of the *Puy de Palinod*, a Rouennais poetry society whose members were involved in preparing the entry, establishes the social and religious preoccupations of the event's organizers. Wintroub goes on to demonstrate some interesting connections among the ancient Roman figure of the barbarian, the medieval wild man, and the New World "*sauvage*" in ceremonial rituals. The penultimate chapter synthesizes the preceding arguments, drawing together many strands of evidence to offer a fascinating, multidimensional view of Rouen's cultural past (in the forms of an established elite, a mythical Golden Age, and an "uncivilized"

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primitivism) and its imagined future (dominated by an emerging new elite, an apocalyptic view of history and an impulse toward commercial and imperial expansion) as they converged on the focal point of the 1550 royal entry.

The royal entry provides a focus for the impressive number of historiographical and interpretive themes discussed here; indeed, the author insists at the outset that his "book wants to be read as a whole" (p. 12). Thus read, however, it raises as many questions about methodology as it does about sixteenth-century Rouen. I find no better way to describe this book's approach than by citing Wintroub's characterization of the early modern wonder cabinet: "Like the *tableaux vivants* of the entry festival, a collection thus appealed to, signified, and advertized the collector's ability to creatively read and analyze the emblematic, associative, and sympathetic histories of its objects across a field of often arcane textual, material, and pictorial associations" (p. 173). As a historiographical method, this approach is at once exhilarating and frustrating: exhilarating in the multiple and varied juxtapositions of cultural phenomena that it allows, frustrating in its potential for arbitrary-seeming associations. Thus, while the unexpected collocation of materials here often yields convincing new insights, some interpretive links are disappointingly weak. It was "perhaps not a coincidence that one of the men chosen by Rouen's city council . . . was Claude Chappuys" (p. 49); "perhaps not a coincidence that all of these men were associated with a [the *Puy de Palinod*]" (p. 68); "perhaps not a coincidence that Mary's eschatalogical role found a terrestrial analog" (p. 140); "perhaps not a coincidence that Francis Bacon employed the triumph to express the social, economic and political power of his great instauration" (p. 253, n. 21). Any one of these "coincidences" taken individually just might have some persuasive power; repeated this often, however, their effect is to undermine confidence in the extent of each one's legitimacy.

The challenge of evaluating this multifaceted work is compounded by the book's omission of original texts, which make but a few rare appearances (and then only in endnotes). I assume this was the press's decision and not the author's; nonetheless, the study's broad scope and innovative approach make it unlikely that any one reader will have the expertise necessary to appreciate the book's often very detailed linguistic arguments in the absence of the French evidence. And if the remarkable range of this book puts heavy demands on the reader, it is not without risk for the author: Wintroub's attempt to bring sixteenth-century Brazilians into "the same conceptual space occupied by Hercules," for example, is undermined rather than supported by his citation of the "explorer of Brazil, Marc Lescarbot" (p. 62). Lescarbot spent several years in Canada but never visited Brazil; moreover, in his *Histoire de la Nouvelle France* (1617), he invokes Hercules in connection with the partially clothed inhabitants of New France (i.e., Canada) as an emblem of their relatively civilized nature in explicit contrast to that of the naked Brazilians.

Despite such occasional errors, one must ultimately admire the ambition and originality of Wintroub's study. On the whole, this is an exciting, thought-provoking, and informative book.

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SYDNEY WATTS. *Meat Matters: Butchers, Politics, and Market Culture in Eighteenth-Century Paris*. (Changing Perspectives on Early Modern Europe.) Rochester, N.Y.: University of Rochester Press. 2006. Pp. x, 232. $75.00.

The historian of food and eating does not generally have an easy time of things. Studying a universal and—usually—daily activity, he or she nonetheless often finds that sources are scarce and the existing historiography is patchy at best. Like any aspect of the history of everyday life, food can seem such a constant that it resists historicization. Even in periods of social and cultural history's ascendancy, the historian of food must deal with the skepticism, perhaps even the mockery, of other scholars. It is not always easy to be taken seriously, when one's next research monograph might involve peanut butter.

Sydney Watts no doubt faced many of these difficulties in preparing her well researched and very clearly written book. While bread and wine both have well-established places within the literature on early modern France, meat has attracted less sustained attention. Composing only a very small part of the peasantry's diet, meat for most eighteenth-century French people was a luxury item. Yet Watts shows that this was hardly the case in the capital: on average, Parisians in the mid to late eighteenth century each consumed 132 pounds of beef and mutton annually. This consumption level, according to Watts, made meat into a paradoxical commodity, one that blurred the luxury-necessity dichotomy and required new forms of policing and administration.

In eight concise, well-structured chapters, Watts examines the eighteenth-century meat trade from the vantage point of consumers and producers alike. By law, cattle and sheep could only enter the capital from the state-regulated markets in Poissy and Sceaux. In her analysis of these markets, Watts shows urban butchers as themselves wily consumers, capable of circumventing official guidelines even as they adjusted to life in increasingly monetized and regulated networks. Elsewhere in the book, we see these same butchers as vendors, men whose livelihood depended on selling every piece of the animals they purchased. Operating in designated meat markets, from stalls within general food markets, and from the few actual butchers' shops allowed within the city limits, they sold roasts (or even entire carcasses) to affluent households, neck and underbelly cuts to artisans, and offal to the poor. According to Watts, the butchers thought of themselves more

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as producers than as middlemen. Their job was to produce meat from the raw material of a living animal.

Throughout her study, Watts positions herself more as a historian of labor than as a scholar of consumption. Her focus is much more on the butchers than it is on the meat. One chapter shows butchers acquiring animals for slaughter, another shows them selling meat. One details guild structure and administrative practices, while another chapter describes the experience and professional prospects of apprentice and journeymen butchers. In the two most innovative chapters, Watts concentrates on butchers' use of familial and credit networks in order to amass and consolidate wealth. Although some of the themes in these chapters will already be familiar to social historians of early modern Europe, it is rare to see such claims fleshed out so fully for a particular group of trades people.

Given the "discursive" emphasis of so much recent scholarship on eighteenth-century France, Watts's book is nothing short of refreshing in its reliance on notarial records and police archives. This is not easy research to do, and Watts is to be commended for her thoroughness and for the clarity of her presentation. Nonetheless, it is hard to escape the feeling that there has been a bit of a missed opportunity here. Watts treats the butchers very much in isolation, even as she emphasizes that business success depended on kin and credit relations. One wonders, at the very least, about interaction between the butchers and the pork butchers (*charcutiers*), who formed a separate guild. Passing references are made to leather dressers and tanners, but had their occupations and social status (or, rather, lack thereof) been more systematically addressed, a clearer picture of the butchers' tenuous cultural grasp on merchant respectability might have emerged.

A highly competent and informative piece of work, this book would make a greater impact were it bolder in its ventures beyond social and labor history. Watts writes evocatively about the "pools of blood and bile" (p. 69) in which butchers worked, but her brief discussion quickly gives way to information about guild regulation and police administration. Both, no doubt, were in part responses to the perceived polluting qualities of the butchering trade, but it seems there is a broader cultural transformation here that merits attention. What did it mean to be a butcher in an age of "sensibility"? What did it mean to be a *French* butcher in a century when the "roast beef of olde England" became totemic? These are just two of the questions this book might have addressed, had it been framed more broadly.

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Michael Sonenscher. *Before the Deluge: Public Debt, Inequality, and the Intellectual Origins of the French Revolution*. Princeton, N.J.: Princeton University Press. 2007. Pp. x, 415. $39.95.

Michael Sonenscher is widely recognized as one of the most versatile historians of *ancien régime* France, an ambitious thinker who writes knowledgeably about subjects as diverse as guild life, social class, natural law theory, and political philosophy. The versatility is on display in his new book, an intellectual history based on intimate familiarity with as wide a range of authors as one can expect to encounter in a single monograph. Unfortunately, Sonenscher's impressive learning, the importance of his topic, and the author's dedication to its intricacies only heighten the frustration of the reading experience.

Sonenscher presents his argument as a new interpretation of the political thought of the French Revolution, especially as represented by Abbé Sieyès. He argues that the abbé's theories of representation, and the problems at which they were directed, can be traced back to a widespread Enlightenment-era concern that "the very properties of public credit that favoured prosperity and stability might also favour the kind of social collapse that could be represented by the image of a biblical flood" (p. 4). The book's title thus evokes not the blissfully irresponsible sentiments attributed to Madame de Pompadour but the "future-oriented speculation about public debt" that characterized intellectual culture (p. 3). According to Sonenscher, writers feared that "social collapse" would come either in the form of a military despotism made possible by deficit spending or a crisis-inducing paralysis made inevitable by creditors' stranglehold on the state apparatus. Intellectual efforts to escape from this "double-bind" can be said to have prepared the way for the complicated representational schemes of Sieyès and those influenced by him.

The book's premise is intriguing, and analysis of contemporary thinking about public debt has the potential to shed new light on a number of themes that have attracted attention recently, including luxury, taxation, commercial expansion, and social hierarchy. Sonenscher's approach, alas, makes it exceedingly difficult to embed his many examples of "future-oriented speculation" into any overarching context. The analysis proceeds in an absolutely consistent but startlingly old-fashioned manner, with discussions of individual thinkers lined up one right after the other, apparently selected according to no criteria other than their having read one another and having mentioned the subject of debt. The lack of a discernible filter produces long-winded chapters (average length: seventy-eight pages) whose function in the overall argument will escape all but the most perceptive readers. The study is bibliographically rich, but authors are rarely situated socially or ideologically. Unhelpful quotations—often two to three words in length—are stripped of context and forced into the directionless narrative of the "double bind" for page after page.

In discussions of well-known figures—for example, François Fénelon, Jean-Jacques Rousseau, and Montesquieu—Sonenscher even neglects to explain why "public debt" is now to be the preferred lens of interpretation, joining and displacing from the front rank

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such concepts as virtue, despotism, commerce, or corruption. (Debt often falls out of the picture entirely in the second half of the book, when his authors are preoccupied by other concerns.) An ominous sign of this conceptual imprecision appears early in the introduction, when Sonenscher says his book will "show what the French Revolution *might begin* to look like in the light of a detailed historical examination of the *range of ideas* and *more ambitious* political theories that, *directly or indirectly*, can be *associated with* the menace *underlying* the phrase *après moi, le déluge*" (pp. 8–9; italics in English added.) The book delivers what it promises here, but indirect associations between ranges of ideas that might begin to look like something can hardly provide a foundation for rigorous rethinking of the French Revolution's origins.

Structural weaknesses are compounded by problems of style, and the editor deserves much blame for this. The author's prolixity is permitted to overwhelm the text, creating amorphous paragraphs (some covering multiple pages; e.g., pp. 154–157), maddeningly convoluted sentences, and unnecessary block quotations. Antecedent-less pronouns abound, and getting to the point never seems a priority. Topic sentences typically defer resolution by introducing multiple possibilities at once. So, for example, we learn that David Hume's meditation on the distinction between absolute governments and free states "raises two real historical and analytical questions. The first is whether the modern alternative to absolute government was, as Edmund Burke argued, already in existence well before 1789. The second is whether a real alternative, as Sieyès argued, had yet to be established, and, once it was, whether, as Hume guessed and Alexis de Tocqueville claimed, it owed rather more than might seem to be the case to the old regime" (p. 39). This book requires rather more patience from its readers than the publisher has a right to expect.

Sonenscher has labored long over these texts, perhaps too long. He offers learned and occasionally original discussions of landmark works in economics and political philosophy. But the trees obscure the forest throughout. Connections that may seem blindingly obvious to the author are not communicated effectively to the reader, and for that reason above all this book disappoints.

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Jean-Marc Largeaud. *Napoléon et Waterloo: La défaite glorieuse de 1815 à nos jours*. Paris: La Boutique de l'Histoire. 2006. Pp. 462. €35.00.

Jean-Marc Largeaud's history of the battle of Waterloo is less about the event and more about its place in modern memory. The book surveys nearly two centuries of reflection on the battle's meaning in the politics of French culture.

As a study of the fortunes of the memory of an episode that marked the downfall of France's once great empire, Waterloo has had a particular appeal, for it permits the exploration of the ambiguities of its remembrance as a "glorious defeat." The vanquishing of Napoleon Bonaparte's army at Waterloo signaled the definitive end of French hegemony in Europe. But its memory lingered in French popular culture, and over time it was transfigured into the idealized image of a courageous last stand, a touchstone of the resilience needed for national renewal. Waterloo, experienced by its soldiers as the existential suffering of agonizing combat, would be remembered by succeeding generations as a disembodied emblem of devotion to duty, loyalty, and patriotism against all odds. So its lessons would be enshrined in popular histories, the schoolbooks of children, romantic novels, and the ritual ceremonies of national commemorations. In these many ways, the losses of the battlefield found recompense in the psychological consolation of memorable lessons for posterity. Ironically, Largeaud points out, this "remembering" of the event received its most grandiose homage during the Third Republic, not the Second Empire.

Largeaud's book is skillfully conceptualized and exemplary in its exhaustive investigation of the many ways and contexts in which the memory of Waterloo has since been refashioned. Much of its interest lies in the rhetorical strategy with which the author has structured his study. His rendering presents a synchronic conception of historical time, as he inventories the remembrance of the event across the range of its possibilities. Largeaud begins with the accounts of witnesses, then traces its passage into collective memory, before turning to the way the event was subjected to the scrutiny of historical reconstruction. Finally—and this may be his most original perspective—he addresses the way in which the history of the event came to be rivaled by its memory once more, this time in the guise of its imaginative recreation. Here Largeaud explores the representations offered by novelists, poets, playwrights, and painters over the course of the nineteenth century, all of whom sought to imagine the battle as it dramatized some deeper issue about the human condition.

Largeaud's purpose is to show how, over the course of the nineteenth and even into the twentieth century, Waterloo's history was appreciated within a larger framework of remembrance. To search for the meaning of Waterloo in modern French culture, he explains, is to turn to its memorialists as well as to its historians. In his presentation of Waterloo revisited, therefore, the work of history is situated between two kinds of collective memory: one that stresses commemorative remembrance, the other the moral imagination. Henry Houssaye's history, written at the turn of the twentieth century, may have been the high point in the historians' reaffirmation of the cult of Napoleon as the nation incarnate. But his account was rivaled in popular appeal by the fictional representations of Stendhal and Victor Hugo, who, in reaching for the historical sublime, turned history into myth once more.

Largeaud shows how each mode of remembering the

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past presents distortions with which today's historians must reckon. Testimony shades into hearsay. Collective memory is continually reshaped by its political uses over time. The popular histories through which the event has been interpreted bear the marks of their authors' biases. Artists, dramatists, and novelists direct attention to more abstract ideals for which the event itself may be little more than a convenient prop.

For Largeaud, what mattered most to successive French generations in remembering Waterloo was the need to find therein edifying meanings to connect to their present circumstances. By the early twentieth century, Waterloo's memory had been largely drained of such possibilities. From the vantage point of military tactics, it had ceased to have any practical value. From the standpoint of the larger fortunes of the nation, its importance had waned with the decline of its revolutionary tradition. While France would know defeat once more at the outset of World War II, there was no sentiment to identify the valor of Napoleon's last stand with the Third Republic's humiliating capitulation. Nor is there any place, Largeaud suggests, for Waterloo in France's contemporary search for its identity within the European confederation. In such circumstances, Largeaud reasserts the primacy of the historians' claim to interpret not only Waterloo as battleground, but also the traditions of remembrance in which it has long been enshrined.

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Denise Z. Davidson. *France after Revolution: Urban Life, Gender, and the New Social Order*. (Harvard Historical Studies, number 155.) Cambridge, Mass.: Harvard University Press. 2007. Pp. vii, 257. $49.95.

The impact of the French Revolution on French society and culture, as opposed to politics, has attracted growing attention in recent years. And whereas once the divisions between periods were all but unbridgeable except in general textbooks, growing numbers of historians are now crossing the academic boundaries between revolution and Napoleonic Empire and between empire and Restoration. When they examine responses to the revolution, furthermore, a lengthening list of books and articles employ new methods of cultural analysis to examine the way gender, space, and politics interact to modify social practices. Denise Z. Davidson's new book takes up these themes and, rather than focusing on Paris, which has been the subject of most work, devotes welcome attention to major provincial urban centers, particularly Lyon and Nantes.

Davidson begins by discussing an essay that won the prize of the Academy of Science, Letters and Arts of Lyon in 1822 and that condemned the deleterious effects of theater on the working population of France. The common people were led to emulate their betters and this led to inappropriate, even dangerous behavior and expectations. The French Revolution, Davidson suggests, created intense anxiety about social hierarchies by dismantling the "symbolic system" of the Old Regime. She is particularly interested in the way that women's behavior became central to the new social and political order that was constructed during the Napoleonic Empire and the subsequent Bourbon Restoration. Like other cultural and social historians, she is concerned to explore the way that gender contributed to the creation of a new class system in nineteenth-century France and to test hypotheses about a growing separation between public and private.

The book is organized around three main themes: political festivals, theaters, and associational life. In all three areas, Davidson is most interested in gender on the one hand, and in the relationship between private and public (including the implications for politics and the state) on the other. Festivals were consciously designed, during both the Napoleonic Empire and the Restoration, to restore solidarity between the people and the government and to recreate bonds between different groups in society that were felt to have been sundered by the upheavals of the preceding period. Many of the tactics were the same: the distribution of food, free theater and music, and the parading of model citizens, particularly women. But the differences are revealing: whereas the empire was very hostile to women's involvement in politics, the Restoration saw women as more attached to religion and monarchy and therefore allowed them a greater role. Their task was "to calm and soothe, to make good the bad that had been done" (p. 49). Yet there was a significant class difference: while the empire selected ordinary women as models, the Restoration initially allowed only aristocratic women a key part. Later, when the Bourbons felt more secure, they invented prizes for virtuous behavior, awarded to carefully chosen women "of the people" who were again given some prominence.

Similar themes are apparent in the theater. Davidson is interested both in the audience and in what was happening on stage. In an informative analysis first of the spectators of Paris boulevard theaters, then of the most popular plays, she suggests that this was one way people of all classes learned to "read and navigate" the new society of the city. Provincial theaters, surprisingly, appear to have been sites of more acute tensions. This may reflect the different sources that Davidson uses for Paris and for other towns, but the anxieties that municipal authorities expressed are very revealing of gender stereotypes that cast women as the principal cause of disorder. Theaters were public spaces but also, particularly at the end of the Restoration, political ones that the authorities found very difficult to control.

The final section of the book examines the key forms of urban associational life in the early nineteenth century: salons, charities, cafés, and balls. All, to varying degrees, were places where men and women mixed, and all were spaces where people were both observers and observed, playing out and constructing postrevolutionary gender and class roles. Yet many of them, the bourgeois *cercles*, the respectable cafés and the more plebeian cabarets increasingly became male establish-

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ments. At the same time they became more socially segregated, contributing to the development of a class society.

Davidson is attentive to the impact of growing consumerism on these various processes, showing how women became the bearers of particular social markers through fashionable display. Women became a barometer for social health, their behavior and morals singled out for particular attention by the police, by journalists, and by social commentators. These are familiar arguments, but what is of particular interest in this book is seeing how the shifts in postrevolutionary gender and class norms played out at the local level, in the French provinces.

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COLIN HEYWOOD. *Growing Up in France: From the Ancien Régime to the Third Republic*. New York: Cambridge University Press. 2007. Pp. xi, 313. $90.00.

When Colin Heywood published his first book, *Childhood in Nineteenth-Century France: Work, Health and Education among the Classes Populaires* (1988), the history of childhood in France was in its infancy. In his new book the history of childhood has reached its maturity, as Heywood's extensive bibliography attests. This book covers not only the "long childhood," roughly from age seven through twenty-five, but also the *longue durée*, from the mid-eighteenth century through the mid-twentieth. The first third of this exhaustively researched study begins with an analysis of cultural constructions and representations of childhood. The second examines the lived experiences of children within the family and among friends, and the third section covers children's lives as they leave their families, attending school, going to work, and marrying.

By privileging a plethora of personal reminiscences of childhood derived from correspondence, oral histories, diaries, autobiographies, and memoirs—what Heywood calls "ego documents"—he allows specialists and generalists alike to share the remembered experiences of famous and obscure French men and women from many walks of life. Heywood's work adds a new dimension to histories of childhood. Previous works, largely inspired by social history, have focused on church and state policies, social programs, and institutions for children, furnishing a history of how adults defined, treated, and provided for children. Heywood acknowledges those earlier histories "from above" but concentrates on history "from below": the experiences of children themselves, albeit remembered by those children when they were adults. Since adults rarely recall their earliest years, Heywood pays scant attention to infancy, a well-covered aspect of the history of childhood that has been the focus of historians of welfare, public policy, motherhood, or medicine.

After an astute discussion of the value of "ego documents," Heywood analyzes eighteenth and early nineteenth-century debates on childhood, which tended to place children in dichotomous categories as evil beings or as angels. He concludes that during this period, the sheltered "long childhood" established by religious schooling was markedly class and gender linked, predominantly for elite males; peasants and workers had remarkably short childhoods, usually ending at age twelve. Moving away from representations of childhood, chapter three draws on theoretical perspectives of philosophers, novelists, folklorists, doctors, and social scientists to help explain how the French understood and construed their own experiences of childhood, revealing how a focus on life stages and on the rites of passage is class and gender dependent. Primarily utilizing the autobiographical novel, Heywood considers life's turning points, such as death of a parent or a religious experience. Almost all the chapters begin with a mention of Jean-Jacques Rousseau, but Heywood rapidly moves on to other writers of the eighteenth through the twentieth centuries, from well-known literati such as George Sand and André Gide to workers and peasants such as Jacques Ménétra and Emilie Carles.

Starting with chapter five, Heywood shifts from the representations of childhood that characterize the first third of the book to a depiction of the growing-up process, first highlighting the historical debate over the similarities and differences between families in premodern and modern times. In subsequent chapters on children's reactions to often harsh and distant mothers and fathers, Heywood demonstrates that children from the rich and famous to the poor and humble were adaptable and resilient; they coped with multiple models of mothering and fathering. Eschewing stereotypes and basing his analysis on a wide range of recorded childhood experiences, Heywood concludes that children from all levels of society received support from kin and community. Reminiscences about food, clothing, housing, and illness are fascinating but all too brief; some readers might wish that Heywood had supplemented these stories with contributions from other literature, such as folklore collections.

The third portion of the book describes the last stage of childhood, privileging the young adult's point of view. Significantly, during the late nineteenth and especially during the twentieth century, the establishment of secondary schools led to the prolongation of childhood for almost all socioeconomic classes. Unsurprisingly, urban boys more than rural boys or urban or rural girls experienced the protected "long childhood," since they had more regular school attendance. Social inequalities persisted, and readers see the grueling hardships of child labor from workers such as Norbert Truquin and Jeanne Bouvier.

Remembrances of childhood, obviously, consist of experiences colored by age and circumstances that adults choose to portray, often designing these memories for their contemporaries. Nevertheless, as Heywood has shown, historians can appreciate them as self-representations, especially in the absence of other personal accounts of childhood. Heywood eschews

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overgeneralizations about childhood in France and instead provides readers with a great diversity of childhood experiences varying by class, gender, regions of France, and even according to individual family dynamics. He allows us to understand better what growing up meant to the children of France.

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THOMAS CRAGIN. *Murder in Parisian Streets: Manufacturing Crime and Justice in the Popular Press, 1830–1900.* Lewisburg, Pa.: Bucknell University Press. Pp. 273. $75.00.

The study of crime and stories about crime opens a window on multiple themes of cultural history. By analyzing the *canard*, a news genre featuring sensational and criminal stories, Thomas Cragin examines the preoccupations and assumptions of popular culture in nineteenth-century Paris, their relation to elite culture, and the differences between traditional and elite perspectives. He argues that the persistent popularity of *canards*—even after the emergence of mass-circulation dailies in the 1860s and 1870s—demonstrates the resistance of popular culture to the intrusion of elite notions and sensibilities.

The *canard*, joining early modern oral culture with the changing print culture of the nineteenth century, presented news via peddlers who sang out the details of horrific crimes to an urban street audience. Part street show, part crime drama, it consisted of two elements: the rhymed ballad performed in the street, and the narrative that elaborated details of the crime, police investigation, trial, and punishment. Consistent with early modern folk tales that relished the rehearsal of gruesome acts committed by human monsters, the *canard* presented a coherent moral universe defined by a contest between good and evil in which justice ultimately prevailed. Produced by journeymen printers and peddlers, often with dual authorship, the *canard* continued to dominate the popular literary market through most of the century.

In part one, Cragin provides a compelling account of the production and distribution of *canards* as new print technology made possible larger runs, and peddlers and printers evaded censorship regulations that were only lightly enforced for nonpolitical publications. Part two explores the highly formulaic mental and moral universe presented in the *canards*: the criminal was, in effect, a monster, separated out from any social group or class; the personal history of the accused—typified by sloth, alcoholism, recidivism for men, and aggression, sexuality, and refusal of traditional roles for women—attested to guilt, as did an evident lack of remorse; the police and judges were the instruments that assured justice; severe punishment, especially the death penalty, was required; an emphasis on violence inflicted on innocent women and children confirmed the monstrosity of the criminal and enhanced the emotional impact of an essentially melodramatic story.

Cragin argues that the uniformity of the content of *canards* over time demonstrates essential continuities in the popular imagination in a period of alleged modernization. The *canard*, in this analysis, resisted new ideas from elite culture about the sources of crime and the nature of the criminal, set the model for the emerging mass circulation press, and preserved notions of justice more in keeping with the mental universe of earlier centuries than with the institutions and practices of the modern state.

In this study, Cragin has effectively animated our images of nineteenth-century Parisian popular culture with his vivid descriptions of ubiquitous *canardiers*. He has demonstrated the robustness of a news genre that anticipated and overlapped the tabloid press of the end of the century, and he has convincingly shown that the modern did not simply displace the traditional. But even within this apparent continuity, it does seem that Cragin has overstated his case, documenting examples of continuity when a more nuanced conception of how meaning is created might have shifted the argument. If, as Cragin notes, *canardiers* revised their ballads in response to the demands of their audience, it would be interesting to know how these demands might have changed over the century. It is possible that the same content, expressed over a period of decades, might imply different meanings and have a different cultural resonance for different generations of listeners. Would the sensationalism of the O. J. Simpson case speak differently to different generations of Americans? I think so.

Cragin's exclusive focus on *canards* as the repository of popular beliefs, set against a less clearly defined notion of elite culture, begs for some qualifications. It would be helpful to know more about the elite culture under discussion; in this work, it serves more as a foil than a set of clear premises. For example, was the analysis offered by criminal anthropologists or forensic doctors less categorical, less focused on evil than that represented in the *canards*? To what extent were women who rejected conventional behavior described differently in elite discourse? Or were these accounts, in the end, quite similar to that of the *canards*? Did the influence travel in both directions? Indictments handed down by the prosecuting investigator were also formulaic. How similar were these formulas to those of the *canard* narrative? Perhaps such a juxtaposition of different kinds of criminal storytelling might reveal more of a symbiosis among genres—one that reinforced familiar stereotypes and testified to a circulation among stories and between cultures in the city. Situating the *canard* more directly in relation to the genres with which it was inevitably in contact might produce an account characterized less by difference and exclusion than by cultural overlap and interpenetration.

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ISABELLE VON BUELTZINGSLOEWEN. *L'hécatombe des fous: La famine dans les hôpitaux psychiatriques français*

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sous l'Occupation. (Collection historique.) Paris: Aubier-Flammarion. 2007. Pp. 512. €22.00.

Between 1940 and 1944, 78,287 people died in French psychiatric hospitals. The number of "surplus deaths"—that is, the number by which the total of deaths exceeded that which might have been expected if peacetime mortality rates had continued—was something between 43,158 and 44,144. Who was responsible for these deaths? In recent years, it was been argued—notably by the psychiatrist Max Lafont—that they were the result of a "gentle extermination" by a Vichy government that had been influenced by eugenicist thinking. Patients were not killed with gas and lethal injections, as they were under the Nazi "euthanasia" program in Germany, but they were deliberately allowed to die by starvation.

On the whole, however, historians have been skeptical about Lafont's work. They have stressed that "surplus deaths" might be explained in lots of ways that do not involve an intention to kill on the part of the authorities. They point out that mortality rose among other groups in France, which do not appear to have been the object of any official animosity, and that food was often short for reasons that did not imply a desire to starve anyone.

Isabelle von Bueltzingsloewen's careful and somber book stresses the difficulty of researching this topic. The testimony of psychiatric patients was less likely to be listened to than that of other people; no one seems to have interviewed patients, or former patients, in any systematic way. Other records are scarce. Archives raise awkward questions: what are we to make of letters from an inmate who believed that the doctors were trying to kill her, if that inmate had been diagnosed as paranoid?

Simply establishing causes of death is harder than it looks. Patients were sometimes unwilling to eat and some had died of malnutrition even before the war, at a time when food supplies seem to have been relatively abundant. Official reports grouped deaths into broad, often unhelpful, categories. Doctors seem to have been genuinely mystified as their patients began to die in such large numbers. They could not accept that death by starvation was possible in France, and they often believed that the problem lay with vitamin deficiency rather than the inadequacy of all food. Statistics are hard to read. We know that the mortality rates in psychiatric hospitals increased, but it is not clear that the kind of patients in them remained the same. Some of the least severely afflicted were removed by their families during the occupation.

Mortality rates varied from place to place; they were lowest in fertile agricultural areas. They also varied with types of patient. Patients who had jobs within the hospitals survived better than those who did not. Patients whose families paid for their upkeep were better off than those who were dependent on public assistance. Mortality rates also varied over time. They went up sharply during the chaos that followed the defeat and reached a peak in 1942. After this they began to decline. All these statistical patterns tend to argue against a policy of systematic extermination. Deliberate killing would, presumably, have operated with more homogeneity across the country and it would not have dropped off halfway through the occupation. Indeed the drop in death rates seems largely to have been due to the fact that the authorities, eventually, worked to prevent deaths.

Von Bueltzingsloewen suggests that death sprang not from a deliberate government project but rather from the fact that inmates of mental hospitals were at the very bottom of a hierarchy of deprivation that, to some extent, affected most French people. Official rations were barely enough to support life during the occupation and, in any case, it was not always possible to obtain official rations. Most people were able to survive by resorting to the various black or grey markets. Inmates of mental hospitals could, on the whole, only work through official channels.

The fate of psychiatric patients in wartime France has never been "forgotten." Von Bueltzingsloewen shows that very high mortality rates in hospitals were recognized and discussed from almost immediately after the liberation. Not everyone who died in the hospitals was obscure. Oddly one of the most remembered victims of this episode is the amnesiac veteran of the Great War, believed to be Octave Montjean, who died at the asylum of Rodez in September 1942. Von Bueltzingsloewen's book recounts this case, and brings up the harrowing stories of many other individuals as well. She does not minimize the suffering in French psychiatric hospitals during the occupation, nor does she seek to deny the moral responsibility that individuals (notably those employees of hospitals who stole food from patients) had for this suffering. She does, however, argue convincingly that this suffering was not the result of a deliberate and systematic policy of extermination.

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PHILIP W. BLOOD. *Hitler's Bandit Hunters: The SS and the Nazi Occupation of Europe*. Foreword by RICHARD HOLMES. Dulles, Va.: Potomac Books. 2006. Pp. xxii, 400. $29.95.

Anti-partisan warfare ranks among the most brutal aspects of Nazi conduct during World War II. What was disguised as a legitimate security campaign in Germany's occupied territories was in large part a cover for a vast enterprise of terror and exploitation aimed at annihilating the Reich's purported "ideological enemies"—Jews, communists, Gypsies, and others—and plundering alleged "bandit areas" for labor and economic resources in the ever more desperate cause of Germany's war effort.

Philip W. Blood seeks to explain the long-term historical origins of Nazi anti-partisan warfare, its place within Nazi policy generally, and how it was applied. His is the first attempt at a wide-ranging historical treatment linking all three themes. His arguments are con-

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veyed extremely effectively, and supported by formidable command of an enormous array of sources encompassing captured records, SS-Police signals, and various types of oral testimony. The book further benefits from a quietly impassioned, vivid writing style, leaving no doubt as to the enormity of the crimes perpetrated.

Indeed, Blood points out, anti-partisan warfare is too mild a term for the "security campaign" the Nazis unleashed across occupied Europe. The campaign was founded, rather, on the concept of anti-bandit warfare (*Bandenbekämpfung*), which vilified its opponents as "gangsters" undeserving of the treatment normally accorded captured enemy combatants. The German military refined and expanded this concept throughout the nineteenth and early twentieth centuries, wedding it to a "Cannae approach" to offensive operations that envisaged the enemy's encirclement and total destruction. *Bandenbekämpfung* reached its apogee in the cause of the Nazis' *Lebensraum* program of ideological extermination and economic and spatial conquest in the Soviet Union and Eastern Europe. The contemptuous terms of reference for *Bandenbekämpfung*'s opponents made it the ideal basis for a security campaign aimed at annihilation and exploitation.

Blood details the directives and developments that eventually placed *Bandenbekämpfung* at the heart of German security policy. The greatest impetus came from Germany's failure to defeat the Soviet Union in 1941, and the need the Nazis consequently felt for a brutally effective means of pacifying increasingly restive occupied areas, even as they sought to exploit those areas with mounting rapacity. This development benefited above all the SS-Police, which already had proved its ruthless mettle in security matters during the campaigns in Poland and the Soviet Union, and which now assumed a particularly prominent role in security policy. That said, the book provides no succour for those seeking to distance the Wehrmacht, at institutional level at any rate, from such murderous initiatives.

The book's second part details the mechanisms by which *Bandenbekämpfung* was implemented from 1942 onward. Blood details the methods of information gathering, the order of battle for the "anti-bandit" campaign encompassing SS-Police, army, Luftwaffe, and native auxiliary units, the gradations of bandit area that the Germans identified, "anti-bandit" training, and, via several case studies, the variations on the Cannae method employed in various operations.

The final section details *Bandenbekämpfung*'s application during the war's latter years, particularly its export elsewhere in occupied Europe. In Poland it was employed more directly than anywhere else in the service of openly genocidal policies. In Southeastern and Western Europe, it was utilized to murderous effect in such critically important regions as Slovenia—the lynchpin, Blood argues, of the eastern and southern battlefronts—and France following D-Day. Blood argues that murderous "security" operations in occupied Western Europe were not aberrations but measures entirely consistent with the murderous precepts of *Bandenbekämpfung* first applied in the East. The final chapter scorns the postwar efforts of men such as Heinrich Himmler's bandit-hunter-in-chief Erich von dem Bach Zelewski, a central figure throughout the book, to dilute their complicity. In fact, while the *Übermenschen* of the SS "anti-bandit" elite proved frailer than Nazi imagery depicted them, sometimes reacting to their murderous duties with a range of psychosomatic symptoms, this did not translate into actual protests against orders during the war itself.

Future scholars need to examine further individual "security" operations—conducted by the Wehrmacht, SS, and others—and how far they were shaped by conditions in the field and the individual attitudes of units and officers on the spot. Some case studies of this type have already been written by scholars such as Michael Geyer and this reviewer, and Blood takes issue with their conclusions. Yet while it remains important, via a case study approach, to see the trees for the forest, Blood breaks new, important ground in better enabling scholars to discern the forest for the trees. Case studies of Nazi security warfare must also consider the influence of the institutional framework in which operations took place, particularly the general doctrines and methods that were favored, and the mentalities of the higher-level commanders who formulated them. Blood has rendered invaluable service in illuminating these issues to a new and unsettling degree.

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Stanislav Zámečník. *Das war Dachau*. Translated by Peter Heumos and Gitta Grossmann. (Die Zeit des Nationalsozialismus.) Frankfurt a. M.: Fischer. 2007. Pp. 435. €10.95.

Given the centrality and notoriety of the Dachau concentration camp, it is rather surprising that the first comprehensive scholarly monograph of the camp's history by a trained historian was not published until 2002. Czech historian Stanislav Zámečník's book, first published in German, then in French and the original Czech, and in English in 2004, is now available in a paperback edition by a major German publisher. The earlier editions are only available as publications of the Dachau survivors association at the Dachau memorial site. In order to fully appreciate this comprehensively researched, clearly organized, and smoothly written monograph, it is necessary to know something about the author and the previous historiography of the Dachau camp.

Zámečník, born in 1922, was arrested for resistance activities at age seventeen and transferred to the Dachau camp in February 1941. That fall he was able to begin work in the camp infirmary, a haven from more lethal outdoor work and a nucleus of resistance activity, where he remained until liberation. After studying history at Charles University in Prague, he began work at

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the Czech Institute for Military History in 1960, focusing on the history of Czech resistance to Nazism. After the repression of the Prague Spring in 1968, Zámečník was prohibited from conducting historical research, but with the help of German contacts and a friendly librarian in Prague, he was nonetheless able to obtain sources and literature to continue work on this book, publishing a few shorter pieces in Germany, notably a brilliant examination of Heinrich Himmler's reputed order to murder all camp inmates before liberation (" 'Kein Häftling darf lebend in die Hände des Feindes fallen,' Zur Existenz des Himmler-Befehls vom 14./18. April 1945," *Dachauer Hefte* [1985]: 219–231), and a memoir of his time in the Dachau infirmary ("Erinnerungen an das 'Revier' im Konzentrationslager Dachau," *Dachauer Hefte* [1988]: 128–143). After 1989 Zámečník incorporated into his study a host of new archivally based research on the concentration camps, such as Johannes Tuchel's dissertation, *Konzentrationslager: Organisationsgeschichte und Funktion der "Inspektion der Konzentrationslager," 1934–1938* (1991), to name only one of the most important. Zámečník has succeeded in integrating these studies based on bureaucratic documents with the hundreds of published and unpublished accounts of the Dachau camp by those who experienced it firsthand.

This book represents the pinnacle of monographic concentration camp histories. Monographic histories of Nazi concentration camps have long remained the province of camp survivors, beginning with Eugen Kogon's *Der SS-Staat* (1946; *The Theory and Practice of Hell*, 1950; both still in print). The international association of Dachau survivors commissioned Belgian military historian Paul Berben to write the first monograph about Dachau, published in 1968 as *Histoire du Camp de Concentration de Dachau, 1933–1945* (published in English in 1975 as *Dachau 1933–1945: The Official History*). Berben's fine narrative focuses solely on the camp itself, however, without consideration of the historical context. Berben's research and documentary appendixes were incorporated into the first Dachau museum catalog, published in German in 1975 and English in 1978, which remained the only widely available book about the camp until the new catalog was published in 2005. Günther Kimmel's article "Das Konzentrationslager Dachau: Eine Studie zu den nationalsozialistischen Gewaltverbrechen," included in volume two of Martin Broszat's project on the history of everyday life in Bavaria, *Bayern in der NS-Zeit* (1979, pp. 349–413), was the first attempt at a scholarly overview of the camp's history. More recent monographs, such as Hans-Günther Richardi's *Schule der Gewalt* (1983, 1995) and Sybille Steinbacher's *Dachau, die Stadt und das Konzentrationslager* (1993), focus on specific aspects of the camp.

Zámečník's monograph is divided into three chronological-thematic parts, covering the phase of consolidation and preparation for war until 1939, the period of military successes until 1941, and the period in which prisoner labor was used for wartime production. Each of these parts is divided into clearly titled sections and subsections that comprise essays on the crucial issues of Dachau's and the entire Nazi camp system's history. Throughout the book Zámečník moves effortlessly between specifics about Dachau and developments in the camp system, making this book a gripping narrative that provides a superb analysis of how integral the concentration camp system was to the Nazi Reich itself. As such it is eminently suitable for lay readers (including advanced undergraduates), but it also represents the cutting edge of research on the Dachau camp's history, masterfully assessing the source material on such questions as the "night and fog" executions in fall 1941, medical experiments, the Dachau gas chamber, and the extent to which an "international" resistance movement in the camp existed. We must hope that a major publisher will make the English translation widely available as well.

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NORMAN J. W. GODA. *Tales from Spandau: Nazi Criminals and the Cold War*. New York: Cambridge University Press. 2007. Pp. xiii, 390. $30.00.

At the end of World War II, some 12 million German soldiers were imprisoned and thousands of Nazi leaders interned. Most POWs were released within a year, and many of the Nazi small fry returned home soon too. Few admitted outright guilt; most thought they were victims of the war or Allied punishment.

But how do you deal with the leadership of a nation that unleashed the worst aggressive and genocidal war in history? The Allies faced this unprecedented challenge, once Adolf Hitler's Nazi Germany invaded Poland, killed civilians in the bombing of London and other cities, and unleashed a war of racial extermination against Jews and other minorities. They decided to set up the International Military Tribunal (IMT) in Nuremberg (and many succession trials) and indicted the worst offenders on charges of conspiracy, crimes against peace, war crimes, and crimes against humanity (e.g., the murder, enslavement, deportation, or extermination of civilian populations). The last charge in particular was a new legal concept that opened up the floodgates of charges of "victor's justice," or applying ex post facto law to defeated enemies (p. 10). This allowed major war criminals to feel victimized too.

The four-power IMT lasted from November 1945 to October 1946. Twelve of the defendants were sentenced to death by hanging, three were acquitted, while seven received unexpected prison terms. Norman J. W. Goda's book is about the fate of these seven prisoners, incarcerated at Spandau in the British sector of Berlin.

This is a masterpiece of sophisticated historical analysis on the intersections among legal history ("the trial of the century" and the administration of justice of the imprisoned Nazi war criminals), Cold War diplomatic

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history (the hastily agreed upon Spandau prison arrangement was a rare survivor in quadripartite cooperation in post-1948 divided Berlin), and the politics of contested memories in postwar Germany. Rudolf Hess received a life sentence and committed suicide in Spandau in 1987 at the age of ninety-three. The legal wrangling for an early release of the "solitary prisoner of Spandau" (p. 232) lasted for much of the Cold War.

Goda's approach to the Spandau inmates is biographical. He devotes cameo biographies to each prisoner with the exception of Baldur von Schirach (released in 1966). Conservatives lobbied incessantly on behalf of Konstantin von Neurath, and veterans' lobbies on behalf of the grand admirals Erich Raeder and Karl Dönitz. The sickly Neurath and Raeder were released early, along with the pathetic figure of Walther Funk. These three early releases came as a result of macabre wrangling over how to deal with the corpses of prisoners who might die in jail, producing a backlash from the German Right. Dönitz served his ten-year sentence and was released on October 1, 1956, the tenth anniversary of the Nuremberg sentences. When the Cold War was heating up over Berlin and West German rearmament, the intransigent Soviets refused to apply his sixteen months' pretrial custody to his sentence. The bitter Dönitz still considered himself the legitimate German head of state. Moscow did not want him to walk a day early, fearing that he might become politically active again and stir up German "revanchism."

The chapters on Albert Speer and Hess are the heart of Goda's book. Speer, who had been Hitler's architect and armaments minister late in the war, would have deserved a death sentence. But his defense strategy of admitting guilt and feigning ignorance about the Final Solution saved his neck. He served his entire twenty-year sentence despite an elaborate lobbying campaign financed by postwar German business magnates who had profited from wartime contracts. After his release Speer wrote three bestselling books and constructed the myth of his relative innocence, relieving all his former business partners of their guilt. Goda is particularly effective in shredding Speer's bald lies. Hess remained the unreconstructed Nazi who maintained his fealty to Hitler at Nuremberg and defended his policies to his final days in Spandau. The Soviets refused to let the paranoid old man walk out early; they insisted Hess represented the living legacy of Nuremberg and the symbol of Allied justice (p. 238). The tedious Anglo-American twenty-year diplomacy for his early release failed. It is recounted in excruciating detail against the backdrop of maintaining four-power cooperation in Berlin.

The maintenance of the elaborate and expensive quadripartite Spandau prison regime for the weird Hess was absurd, concludes Goda (p. 274). Yet Soviet intransigence to lessen the sentences of these war criminals may well have been justified, for too many German war criminals did not serve out their just sentences and fed the mythology of the "clean Wehrmacht" and German victimization and innocence.

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KONRAD H. JARAUSCH. *After Hitler: Recivilizing Germans, 1945–1995*. Translated by BRANDON HUNZIKER. New York: Oxford University Press. 2006. Pp. xiii, 379. $35.00.

Konrad H. Jarausch poses the question: how, after Adolf Hitler, could "the Germans" be transformed into a "civilized" nation? Jarausch is not satisfied with what he sees as the restricted answers given by an institutionally focused political science literature, or the allegedly largely descriptive narratives of historians. Instead, he claims we need a "cultural approach" that will bring together the levels of high politics and grass-roots experience. Jarausch aims thus both to deflect attention away from an obsession with Hitler and to analyze politics in terms of "socio-cultural experience."

Jarausch is personally very well placed to address these tasks. A native German with an institutional foot on each side of the Atlantic, he is abreast of the current literature in both German and Anglo-American circles. He has played an active role in many of the debates he summarizes, often referring to his own previous monographs, articles, and edited volumes. Copious quotations from contemporary sources—insights garnered from the Walter Kempowski diary archive predominate for the earlier postwar years, newspaper clippings for later decades—are marshaled to bolster an account firmly rooted in current debates, and seeking in some ways to match recent works by Heinrich August Winkler and others.

Jarausch proposes the thesis that the "Westernization" of West Germans—in a more general sense than merely "Americanization"—was a product of interaction between occupiers and occupied, constantly negotiated and transformed in a changing global setting, with key moments of change in the 1960s. While East Germans were constrained for decades by the restrictive dictatorship imposed on them by the Soviet hegemonic power and the ruling communist party (SED), West Germans fought their way through a series of generational and cultural revolts to create, ultimately, a form of civil and "civilized" society, defined by Jarausch in terms of respect for human rights. In the German Democratic Republic (GDR), tiny groups of civil rights activists eventually played a role in the 1989 revolution that overthrew the communist regime; yet their voices were submerged in the further Westernization of unified Germany after 1990. Unification presented a new watershed, but the struggle for civil society is by no means complete: xenophobic violence at home, difficulties in facing the new challenges of adopting an active military role abroad, particularly among ultra-pacifist "post-nationalists" who had learned the lessons of their past only too well, and wider challenges of globalization remain.

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Despite Jarausch's earlier gestures toward postmodernist ruptures, a quite familiar grand narrative is imposed. East and West German history are treated largely separately, with considerably less space given to the East than the West, and there is little depth to the analysis of the GDR (odd, given Jarausch's expertise in this area). Although difficulties in definition of theoretical concepts are explicitly raised (such as "normalcy"), the discussion is not sharply analytical throughout. The quotations from diaries and newspapers are generally deployed for illustrative purposes, rather than energetically engaged to work up new insights and hypotheses. Well-known voices are repeatedly called on for testimony, and after the early years, those who are given a hearing are overwhelmingly male, such as prominent politicians and public intellectuals; very few female voices creep in. Broader reflections on women's experiences are summary at best: a brief paragraph, for example, on the West German "women's movement," introduced as "another positive change" (p. 176); and a comment on a claimed "absence" of any comparable "sexual revolution" in the GDR, which supposedly "added to the difficulty of integration after reunification" (pp. 180–181). Although in a descriptive sense Jarausch's book is a useful survey, it is difficult to see quite how major transformations in German "culture," in the broad anthropological sense, can be explained in this way.

Jarausch raises an important set of issues, and he clearly cares about his subject. There are frequent value judgments, with liberal sprinklings of terms such as "unfortunately," "doubling the vexation" (p. 277), and "exemplary" (p. 279); and the book ends with the plea that "the civilizing of politics ought to be understood as a goal for which one must continually strive" (p. 281). These fundamental concerns may be significant, but complex issues are often brushed over rapidly. Despite his evident liberal sympathies, Jarausch too glibly slides, for example, over the issues underlying what he calls the "double burden of the dictatorial past," particularly with respect to the Holocaust. Similarly, with little apparent consideration of the costs for economically less fortunate members of society, he simply carps that "the collective security reflex all too often prevails over the willingness to assume individual risks" (p. 277). The author's opening claims are, in short, never fully delivered on. The rapid summaries of key developments may nevertheless be helpful to students; and the book certainly exemplifies the kinds of concerns and assumptions prevalent among public intellectuals in today's Germany.

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MARK E. SPICKA. *Selling the Economic Miracle: Economic Reconstruction and Politics in West Germany, 1949–1957.* (Monographs in German History, number 18.) New York: Berghahn Books. 2007. Pp. xvi, 288. $90.00.

The West German "economic miracle" has long been a source of scholarly fascination. How did a country devastated in 1945 rise to the heights of prosperity in the 1960s? Was there anything particularly "miraculous" about the Federal Republic of Germany's (FRG) economic recovery? In this fine book, Mark E. Spicka is interested less in conclusively answering these questions himself than in determining how the Christian Democratic Union/Christian Social Union (CDU/CSU), the chief political beneficiary of West Germany's renewal, addressed them. Chancellor Konrad Adenauer and Economics Minister Ludwig Erhard spent much energy in the 1950s honing a winning political message that combined anticommunism with a growing embrace of mass consumerism. They asserted themselves and their party as the chief protagonists in a marvelous story of economic revival and, in turn, claimed increasingly large victories in the three successive elections of 1949, 1953, and 1957.

On the surface, much of Spicka's story is familiar. We have long known how the CDU/CSU presided over the FRG's transition from an economy of scarcity after World War II to one based on home ownership, high savings rates, and the increasing availability of goods and services. But the author considers closely how this standard narrative was itself scripted. In 1949, the CDU/CSU still embraced a Christian, anti-materialist philosophy and relied heavily on the old base of the Catholic Center party for its narrow success. Its campaign methods were likewise a throwback to the Weimar years. Over the next eight years, however, the party availed itself of the sophisticated tools of modern propaganda to widen its political base and broaden its message. The Christian voter gave way to the consumer—often coded female—as the primary object of conservatives' attention, and the CDU/CSU increasingly based its publicity less on ideology than on the magnetism and authority of Adenauer and Erhard. It considered its two leaders to be brands that needed to be "sold" like any other products. By making use of market research, public opinion data, and advertising, the CDU/CSU was miles ahead of the Social Democratic Party (SPD). It turned to the United States for inspiration, mimicking its ads and posters (long on informative text, short on splashy visuals), its interest in voter psychology, and its pre-testing of campaign slogans and images. Although German business had experimented with these techniques in the Weimar and Nazi years, it was not until the 1950s that they became part of the political landscape.

While the CDU/CSU made effective use of propaganda, it nonetheless faced the challenge of convincing West Germans, particularly working-class voters, that market economics served their interests. With the help of business leaders and advertisers, conservatives portrayed the SPD as the champion of "Asiatic" communism and the CDU/CSU as the agent of economic growth, freedom, and choice. By selling the "social market economy," the ruling parties promoted themselves as defenders of humane capitalism against Soviet-style

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command economics, and in turn helped solidify West Germans' self-understanding as *Wirtschaftswunderkinder,* or children of the economic miracle. To be sure, economics was not the only basis of the CDU/CSU's successes. The discontent of East Germans and Adenauer's embrace of the Atlantic community also accounted for the CDU/CSU's appeal in the 1950s. Not least was the Social Democrats' political backwardness. When the SPD suffered a crushing defeat in the 1957 election, it was forced to revamp its electoral methods and broaden its message beyond class-based politics. This, in Spicka's reading, was essential to the SPD's own securing of power in the 1960s.

Spicka draws on a wealth of published and unpublished sources to trace in compelling detail the CDU/CSU's careful managing of its public image through the application of American techniques. His claim, however, that West German political culture became "Americanized" in the 1950s feels strained. While styles of campaigning may have originated from across the Atlantic, deeper debates over the meaning of capitalism and the legacy of National Socialism persisted in West German politics. Arguably, there was also a greater consensus in the United States about the limits of government intervention and the economic and cultural value of mass consumption. One may wonder, too, whether the CDU/CSU's turn away from anti-materialism and its celebration of consumerism in the 1950s was a more contested enterprise than this book portrays. These concerns notwithstanding, Spicka has made a valuable contribution to our understanding of West Germany's first decade. He reminds us that national myths about prosperity and security do not simply derive from the public's experience of economic well-being. They are also the handiwork of strategists who recognize the political value of a good story.

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Benita Blessing. *The Antifascist Classroom: Denazification in Soviet-occupied Germany, 1945–1949.* New York: Palgrave Macmillan. 2006. Pp. xvi, 288. $65.00.

In the third chapter of this book, Benita Blessing recounts the printing of an unauthorized satirical newspaper by teachers at a school in Leipzig for Christmas 1947. One article mischievously compared the earlier treatment of German soldiers with the present-day treatment of *Neulehrer* ("new teachers") in Soviet-occupied Germany. Both had to be "kept occupied" ("even when no fruitful occupation is available") and were subject to officious control from above in the shape of Wehrmachtofficers and the Department of Education respectively (pp. 85–86). This vignette aptly illustrates the tensions between Nazi past and "antifascist" present that—even when only addressed indirectly or jokingly (as was the case here)—overshadowed political discourse in the Soviet zone between 1945 and 1949. As Blessing's fascinating book reveals, the reconstruction of the school system in East Germany after World War II was an essential part of the struggle to reclaim a "good Germany" from the ruins left behind by National Socialism.

Her book is organized in seven thematic chapters, the first of which examines the central role that antifascism played in the history of the Soviet zone. Blessing emphasizes that the "antifascist school" was not simply a Soviet policy imposed by Moscow-backed "puppets" but drew on progressive German pedagogical ideas dating back to the Weimar era and before and was supported by a variety of educational reformers, not merely Communist Party members (pp. 31–35). The second and third chapters examine the difficult process of setting up and physically rebuilding the "new schools." Subsequent chapters tackle gender roles among the "rubble children" attending school; the postwar remaking of the image of fathers in children's minds; and how the nascent education system was used both to establish a coherent, forward-looking antifascist narrative out of the wreckage of modern German history and to salvage useable "traditions"—such as the works of J. W. von Goethe, the 1848 Revolution, and even Christmas—from this compromised national past. The book concludes by briefly examining the post-1949 period, when the "antifascist school" was supplanted by an overtly Marxist-Leninist curriculum and young East Germans found themselves grappling with a new socialist "nation" that engendered within them both fear and pride.

There is much to recommend in this study. Blessing draws on numerous interesting sources, including children's essays (a particularly valuable find), the diaries of Victor Klemperer and Brigitte Reimann, reports from Soviet zone educational administrators (both Germans and Soviets), journals and newspapers, posters, and even films. Her balanced and lucid conclusions echo recent, and increasingly sophisticated, historiographical trends on the immediate postwar years in East Germany. She explains, for example, that "administrative lines of authority in the Soviet zone educational system were blurry" (p. 50), precisely the mixture of Soviet interference and German autonomy first detailed by Norman Naimark in his landmark 1995 study, *The Russians in Germany: A History of the Soviet Zone of Occupation, 1945–1949.* Blessing's work possesses a nuanced and sympathetic approach to its chief subject matter, one that reflects the complex, often troubled experiences of young East Germans during and after World War II both in personal terms (the loss of family members and homes) and on a wider political scale (the transition from "Hitler fascism" to antifascism and eventually communism). As she poignantly notes, young people attending the "new schools"—which were often cramped, cold, dilapidated, and lacking in educational resources—faced huge pressure from their elders, who regarded them as potential saviors of the German nation but also feared that they would fail to bear the heavy responsibility for "reconstruction" resting on their shoulders (pp. 96–98).

Blessing's study fails to address in sufficient detail the

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relationship between school authorities—in this period not always communists—and communist-led organizations with a direct interest in educational affairs, including the party itself and the "antifascist democratic" youth organization, the Free German Youth (FDJ). It would have also been instructive to see her insightful narrative placed more fully within the context of the excellent literature by the likes of Helmut Schelsky and, more recently, Alexander von Plato and Dorothee Wierling on the "Hitler Youth generation," whose youngest members (born in the early 1930s) fall within the range of her book's interests. This generation seems to have participated proudly and dutifully in postwar reconstruction yet sometimes struggled to leave the past behind. What were the values and chief characteristics of the younger part of the "Hitler Youth generation" in the Soviet zone? To this end, a somewhat lengthier concluding examination of the post-1949 era would have been useful.

It should be emphasized that these are minor concerns. Blessing has written a thoroughly researched, well-argued, and absorbing book on a subject of enormous significance to our understanding of the origins and early development of communist East Germany. For anyone interested in how the "antifascist classroom" shaped this process, it is essential reading.

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Jeannette Z. Madarász. *Working in East Germany: Normality in a Socialist Dictatorship, 1961–79*. New York: Palgrave Macmillan. 2006. Pp. xi, 206. $85.00.

In terms of its political stability, the history of East Germany may be divided into three phases. In the first, structures of rule were established, and movements that challenged the priorities of the Socialist Unity Party (SED) elite were repressed. In the second, commencing in 1961, the SED's rule was consolidated, and disgruntled citizens, having failed to inflict defeat upon the government in the mass revolt of 1953 and unable to leave the country after the sealing of its borders, had little option but to accommodate. In the final phase—the transition to which began, arguably, with the harsher international-economic environment of the late 1970s or, alternatively, with Mikhail Gorbachev's accession to power in the mid-1980s—the East Berlin regime's ability to maintain existing structures narrowed, economic and social conditions deteriorated, and revolution ensued in 1989.

If the distinctions among these phases are played down the story can appear as one of ineluctable decline, featuring a totalitarian regime that engaged in a protracted struggle to bludgeon a reluctant citizenry into submission, a campaign that did nothing but exacerbate grievances among the populace, who thenceforward only waited for the opportunity to revolt. Jeannette Z. Madarász takes a different tack. Her guiding assumption is that the three phases are distinct; her emphases are upon the complex and cross-cutting relations of power within East German society and, relatedly, upon the stability (or "normality") that characterized it for eighteen years following the construction of the Berlin Wall.

Drawing on detailed research in the archives of five factories, Madarász depicts a differentiated power structure, involving company managers and functionaries of central and local party and government, in which interests converged and diverged. She devotes much space to corporate social policy, and to managers' zealous attempts to meet plan targets and to encourage a corporate culture to take root. Some of the techniques deployed toward the latter end—including slogans such as "not possible is not an option"—sound arrestingly familiar to Western ears. Others were more distinctively Soviet: for example, the organization of welfare and cultural opportunities at the factory level. One factory, which enjoyed privileged status thanks to its export obligations, provided and coordinated for its workforce not only a kindergarten but also a sewing room, hairdresser, holiday accommodation, holiday camps for employees' children, a folk-dance group, an orchestra for mandolins and accordions as well as a general orchestra, and a library containing around 8,000 books (p. 56). Such luxuries were, however, the exception. In another factory examined by Madarász conditions were appalling. The workforce endured a temperature range of 0–60°C, and a disturbingly high proportion regularly faced exposure to toxic chemicals. (As compensation for the risks involved employees received a small bonus, and those who demonstrably suffered from the effects were given free milk.)

From her research in company and trade union archives, Madarász has unearthed a wealth of interesting material. For example, the welfare improvements of the early 1970s were contemporaneous with an extension of the three-shift system in industry that sparked a jump in rates of absence from work. In the same years, we learn (p. 164)—to this reviewer's surprise—that the permission granted West Germans to visit East Germany during official holidays prompted much grumbling among easterners, on the grounds that any of their western relatives were free to drop in on them, while they were permitted no choice as to whom they wished to see. Similarly counterintuitive is the disclosure that, at least in the 1970s, SED members were as reluctant as were non-members to bring problems to the attention of functionaries (p. 154).

In places, Madarász's grasp is rather less assured. For instance, the suggestion that (p. 99) "individual performance counted only for 20 to 30 per cent of an employee's wages, thereby undermining motivation" contradicts the reality that this figure is very high by international standards, and ignores the experience of the East German workforce in the 1940s, when high rates of performance-related pay undermined morale and motivation. On a range of issues the explication of concepts and presentation and defence of hypotheses would benefit from greater precision. What does it mean to say that (p. ix) "fundamental to East German

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society was the search for a collective spirit"? What exactly are processes of "normalization" and "individualization"? Why is it senseless to distinguish between "perpetrator and victim" in East Germany (p. 9)? Also, if the thesis that workplace-level policy was a vital stabilising factor in the 1960s is to convince, should not alternative hypotheses—such as the role of repression—be considered too?

Despite its shortcomings, this book is a welcome addition to the literature on industrial relations and social policy in Soviet-type societies, and a useful sequel to Madarász's previous volume, *Communication and Compromise in the GDR, 1971–1989* (2003).

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Petr Mat'a and Thomas Winkelbauer, editors. *Die Habsburgermonarchie 1620 bis 1740: Leistungen und Grenzen des Absolutismusparadigmas*. (Forschungen zur Geschichte und Kultur des östlichen Mitteleuropa, number 24.) Stuttgart: Franz Steiner. 2006. Pp. 474. €59.00.

Central though it was to the European state system from the sixteenth century on, the Habsburg Empire has not figured prominently in discussions about the applicability of the term absolutism to early modern monarchies.

Unlike France, England, and even Spain, the archives of the Habsburg Empire's successor states were often awkward to use under the various dictatorships of the twentieth century. At worst, they were sometimes open only to scholars both domestic and foreign who could be counted on to reach politically acceptable conclusions. Full examination of the Habsburgs as monarchs of Bohemia and Hungary did not really get underway until well into the 1990s, but since then a very dedicated group of Czech, Hungarian, Austrian, and German early modernists has worked prodigiously to overcome this deficit. Petr Mat'a and Thomas Winkelbauer's edited volume can be read as a provisional synthesis of a field in progress. The excellent footnote apparatus that each essay carries is less a guide to archival holdings—although those are identified when appropriate—than to the developing secondary literature in German, Czech, and Hungarian on the character of Habsburg rule throughout major centers of the empire as a whole.

Though yet another collection of conference papers, Mat'a and Winkelbauer's volume is atypically coherent. With one exception—an informative but relentlessly parochial analysis of the Hungarian counties—all contributions either confront the question of absolutism and Habsburg rule head on or deal with matters that any historian is likely to take up in covering the issue. Winkelbauer's study of Habsburg finances and fiscal practices in the early modern era and Michael Hochedlinger's essay on contemporary military institutions (and impassioned brief for the primacy of *Machtpolitik* in the seventeenth and eighteenth centuries) are outstanding examples. Katrin Keller's examination of the females invited to attend the households of Habsburg consorts in the sixteenth and seventeenth centuries is a thoughtful and wide-ranging account of a practice that exemplified the Habsburg style of integrating nobilities into their courts while at the same time acknowledging their status. Stefan Samerski's contribution on the efforts of the dynasty to all but sanctify itself, a useful attribute certainly when fending off Ottoman expansion, would also have a place in any discussion of Habsburg absolutism, however conditional it may have been.

Some authors are reluctant to abandon the concept of absolutism, if only because it serves as a yardstick to measure the progress of Habsburg centralization, an ongoing process throughout the early modern era. Jaroslav Pánek, for one, regards the efforts of Ferdinand I to bring the government of Bohemia under closer royal control as an unfinished product. Nevertheless they strengthened the hand of the monarch in ways that are most conveniently described as absolutistic.

Most of the articles, however, are driven by a confident revisionism that upends some of the most widely held clichés of the field. Hungary is commonly cited as the arch-example of resistance to Habsburg manipulation of indigenous custom and privilege. Géza Palffy's piece on Hungarian local and military administration in the sixteenth century suggests that the Habsburgs influenced the fiscal and military structure of that kingdom more extensively than traditionalist, particularly nationalist, historians care to admit. Tomáš Knoz makes a very effective case for revisiting the land confiscations of Emperor Ferdinand II in Bohemia during the Thirty Years' War. Once and still presented as an example of Habsburg despotism, the policy, Knoz points out, was welcome among certain elements of the Bohemian population.

Indeed, if there is a general theme to be developed from the book, it is that self-interest persuaded Habsburg rulers and their constitutional whistle-blowers, usually local estates of their various lands, to cooperate rather than to clash. The nuances of such interaction in the Kingdom of Bohemia are explored very productively by Mat'a and Eduard Maur. The latter, for example, points out that Emperor Leopold I's attention to peasant unrest in Bohemia in the late seventeenth century was not unwelcome among landholders, who realized that a belligerant labor force was the last thing they needed. Like most of the contributors, however, Mat'a and Maur are reluctant to pin comprehensive labels of any kind on Habsburg government in the early modern era. An exception is Karin MacHardy who, after the clearest general discussion in the book of monarchical power in early modern Europe generally, and the Habsburg lands in particular, coins "co-ordinated state" to describe the symbiotic relationship between the dynasty's territorial rulers and local estates to which many of her co-authors refer. Useful as it might have been to conceptualize these findings, she qualifies the term so extensively that its hermeneutic force disintegrates.

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On balance, the volume tells us that the term "absolutism" covers very little of the behavior of the Habsburg monarchy in the early modern era; what there was of it, as Joachim Bahlcke points out, was confessional, rather than political. Even here, or at least in the case of Counter-Reformation Bohemia, analyzed by Alexander Catalano, the dynasty had its own version of the political-institutional side of Catholicism that did not always square with positions taken by Bohemian prelates. For fiscal reasons alone, as Mark Hengerer points out, Habsburgs were way too dependent upon the resources and services of their estates—in this case in Lower Austria—not to adjust their centralizing ambitions accordingly.

If the collection itself has limits, they lie in the neglect of the comparative dimension to this story. Mat'a and Winkelbauer make a point of this to justify including the early modern Habsburg monarchy in the general discussion of early modern political institutions. Additional examples of such work in this volume would have made the editors' argument more persuasive. Jeroen Duindam's discussion of the courts of Vienna and Versailles, covered more thoroughly in his monograph on the topic, talks very intelligently about the usefulness of comparative history but does not dig particularly deeply below the surface behavior of the two courts. If the other essays in this collection mention non-Habsburg polities, it is in passing and not really central to any given contribution. There is, however, enough material to convince Europeanists that current scholarship on the nature of the Habsburg monarchy in early modern Europe is not only alive, but very expertly served.

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DIANA ROBIN. *Publishing Women: Salons, the Presses, and the Counter-Reformation in Sixteenth-Century Italy.* (Women in Culture and Society.) Chicago: University of Chicago Press. 2007. Pp. xxvi, 365. $45.00.

Diana Robin is a scholar of Renaissance Italy whose publications include edited translations of texts by Laura Cereta, Cassandra Fedele, and Isotta Nogarola in the series "The Other Voice in Early Modern Europe" (1997, 2000, 2004). In this monograph, inspired by Carlo Dionisotti's suggestive essay "La letteratura italiana nell'età del Concilio di Trento" (1967), she examines "the very public emergence of women writers as a group for the first time in Italy, during the years 1530–70" (p. xvii). She focuses on a series of female-led salons in Naples and environs, Venice, Rome, Siena, and Florence, where women and men gathered to discuss and produce literature. Serving as promoters and editors, men in these groups saw works by, addressed to, and about women through the press.

Unlike such laborers in this Italian vineyard as Virginia Cox, Ann Rosalind Jones, Victoria Kirkham, and Deanna Shemek, Robin is not concerned with the formal aspects and quality of her subjects' production: for the most part, in her presentation, Petrarchan sonnets. Rather, she concentrates on the particular social, political, and religious circumstances that sparked this outpouring. Except for the Sienese, all the women who feature in her study were lay members of the nobility, several of whom in widowhood lodged at least part-time in convents. From less illustrious backgrounds, their male associates rose to prominence through their literary activities, association with prominent lay patrons (including princes), employment in presses, and in some instances ecclesiastical careers. On the political front, most of the women worked actively, though ultimately in vain, to defend their families' territories or their native cities from papal and imperial incursions. Witness Vittoria Colonna's fierce epistolary battle on behalf of her brother Ascanio against Pope Paul III during the Salt War of 1541 and Laudomia Forteguerri's leadership of female teams that labored to reinforce the defensive armature of Siena during the assault by troops of Charles V and Cosimo I de' Medici in 1555.

Robin persuasively situates most of these women and men in the religious current once known as "Evangelism"; that is, to use the more appropriate contemporaneous term, they were *spirituali*. Influenced by such charismatic figures as Juan de Valdés and Bernardino Ochino and exposed at least indirectly to the ideas of northern Protestants, they endorsed justification by faith alone and called into question other doctrines, practices, and behaviors promulgated and exhibited by churchmen. Vittoria Colonna, Giulia Gonzaga, and Caterina Cibo associated closely with men of a like religious persuasion: Ochino, Pietro Carnesecchi, and Marcantonio Flaminio. For years, these women were objects of surveillance by intransigent opponents of religious dissent, above all Gian Pietro Carafa and Michele Ghislieri, key figures in the Inquisition before their elevation to the papacy as, respectively, Paul IV (r. 1555–1559) and Pius V (r. 1566–1572). Although Colonna, Gonzaga, and Cibo were not put on trial by the Holy Office, both during their lifetimes and after their deaths their names cropped up frequently in trials of their male friends. That the pontificate of Pius V coincides with a precipitous decline in books entirely devoted to and anthologies containing works by women is no accident. The full flowering of the Counter Reformation brought this season of publication by Italian women to an end.

Robin's lively, informal prose style makes her compelling narrative a good read. Probing beneath the surface, however, this reviewer encountered several serious problems. Typographical errors of all sorts—for which author and publisher share responsibility—have become, alas, so common in scholarly books that the many in this one do not merit extended discussion. More troubling are other flaws. Factual errors occur with such frequency as to erode the author's credibility. Marino Sanuto's *Diarii*, for instance, were not published in the sixteenth century, as Robin suggests (p. 13), but in the nineteenth and early twentieth. Pier Martire Vermigli was prior of the Lateran Canon house in Lucca, not bishop of that city (p. 80). José Nieto, whose

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book on Valdés appeared in 1970, is no longer "the leading authority" (p. 278) on this seminal figure.

This last example points to a fundamental shortcoming in Robin's book: her use of studies by other scholars. Rather than seeking out the most recent historical treatments, she relies primarily on obsolete works. One simply cannot produce a credible treatment of Giulia Gonzaga on the basis of Mario Oliva's semi-popular biography (1985). Direct engagement with Massimo Firpo and Dario Marcatto's edition of Pietro Carnesecchi's Inquisition trials (1998–2000)—as opposed to her few endnote references to it—is absolutely necessary. For some unstated reason, Robin dismisses the current consensus that the most important text produced by the *spirituali*, the *Beneficio di Cristo* (1543), was drafted by Benedetto Fontanini da Mantova, a Benedictine monk, and revised for publication by Flaminio. Instead, she distorts Thomas Mayer's suggestion that Reginald Pole had a hand in its composition into the flat assertion that he and Flaminio were the *Beneficio*'s co-authors.

Robin quotes copiously from poems and letters, with the original Italian following her translations. In several instances, her English renderings are inaccurate. The most egregious example occurs in a letter of May 4, 1549, from Flaminio to Cibo. Robin renders its opening phrase, "Bench'io sia ancora molto debole del capo e de tutta la persona," as "Although I may not be very weak in the head and my entire body" (pp. 189–190). In fact, Flaminio writes exactly the opposite: that he is still very weak in mind and body.

Robin conducted her research mainly in the extraordinarily rich collection of sixteenth-century Italian books at the Newberry Library. She gives pride of place to a series of poetry anthologies held there, issued between 1545 and 1560 by the prominent Venetian printer Gabriel Giolito, which she indexes and describes in two appendixes. Attention to another genre—letter collections published in the sixteenth century, many of which include some missives from and to women—would have strengthened her argument. Perhaps the most intriguing of these is *Lettere di molte valorose donne* (1548). Not held by the Newberry but available in eleven other U. S. libraries, it is now considered to be a complete fabrication by the prolific philo-Protestant writer Ortensio Lando. As much as if not more than "genuine" literary productions, it testifies to the insatiable taste for women's writing in this period.

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Mark Seymour. *Debating Divorce in Italy: Marriage and the Making of Modern Italians, 1860–1974.* (Italian and Italian American Studies.) New York: Palgrave Macmillan. 2006. Pp. x, 290. $69.95.

The nine dense chapters of this book offer not only a rich reconstruction of the more than a century long and tormented history of the debate over divorce in Italy; they are a precious contribution to overall Italian political history and in particular the history of the state-church relationship in that country. What began as a material conflict over territory transformed in a permanent tension over powers of regulation in matters concerning the family and sexuality.

For the Italian legislators, the introduction of civil marriage was a necessary step in the establishment of the new nation-state, in the creation of an Italian civil society, and in breaking the Catholic Church's monopoly over the regulation of family matters. But this revolutionary break was limited in two ways. First, it did not set a precedence rule between civil and religious marriage. As a consequence, a large segment of the population continued to marry only in church, even if it meant that their children remained illegitimate from the point of view of civil law. This problem was eventually solved with the victory of the church in the Lateran Pact (later incorporated into the Republican Constitution), according to which religious marriage had (and still has) civil validity. Second, the new Italian state did not acknowledge the logical consequence of defining marriage as a contract, in so far as it stipulated that civil marriage was indissoluble. The founding fathers themselves, in fact, were not only conservative in family matters, as were most of the European bourgeoisie of the time. They were also unsure of the state's power of influence on a nation still highly fragmented, under threats of secession (from the South), whose population did not have voting rights and was largely illiterate. Introducing a civil marriage largely similar to the religious one in its consequences, without even offering the option of annulment (which was open, under certain circumstances, to those who entered into a religious marriage), was the compromise that set the tone of state-church relationships for the years to come, as Mark Seymour sums up in his conclusion.

The paradox that emerges from Seymour's work, in fact, is that the battle against divorce was the building ground of political Catholicism, mainly through the Opera dei Congressi, eventually opening the way to formal political participation. Well before the so-called "social encyclical," Leo XIII's *Rerum Novarum*, which called for Catholic social engagement, the battle against divorce from the 1880s onward mobilized the church and Catholics as an organized group in the political sphere. It was a victorious battle for many decades, notwithstanding the growing support divorce received in Parliament and even in government. To be sure, as Seymour documents, the complexity of the Italian parliamentary procedure, the short life of its governments, and the fractiousness of the divorce law supporters themselves are to a large degree responsible for the failure to approve such a bill. But the capability of Catholic organizations to put pressure on Parliament and to monopolize public discourse on the family was very great, and the lesson was learned all too well by political parties, including those on the left. Not making an issue of divorce and not interfering with the church's regulation of family matters was a strong concern of the Communist Party after World War II. For this reason,

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family law (in the form it had taken under fascism) was not touched until 1975 and, even if the indissolubility of marriage was not included in the constitution by a narrow vote, divorce became an issue on the political agenda only in the 1960s, when the social and cultural climate in Italy was greatly changed. Furthermore, the Christian Democrats were in government and both their socialist allies and the Communist Party were very cautious in supporting the bill. But the changed social climate weakened the efficacy of the church and the Catholic political and social organizations, and Catholics themselves were split over the issue. This was the first real victory of the Italian state over the church, followed a few years later by that concerning the legalization of abortion. But, more than Seymour seems to suggest, it was a temporary victory in an apparently never-ending battle over the regulation of the family in Italy.

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NANCY M. WINGFIELD and MARIA BUCUR, editors. *Gender and War in Twentieth-Century Eastern Europe.*(Indiana-Michigan Series in Russian and East European Studies.) Bloomington: Indiana University Press. 2006. Cloth $65.00, paper $24.95.

Despite a relatively rich literature concerning the roles of women in World Wars I and II in Western Europe, almost nothing exists on this topic in English relating to Eastern Europe. Nancy M. Wingfield and Maria Bucur's edited collection of essays represents a first step in filling that gap for both students and scholars. Although the individual selections succeed to varying degrees and no collection can cover every topic, most are quite good and the volume has a unity and coherence not always found in collected essays.

Wingfield and Bucur's introduction sets up the major issues to be addressed, including their challenge to the concept of the fighting front as masculine and the home front as feminine, as well as the selective and gendered memory of the wars which often involved the silencing of women's voices. Here, too, they describe their organization of the ten essays into three thematic sections.

The first section, "Challenging Gender Roles/Restoring Order," concerns the relationship between politics and stereotyped gender images. Alon Rachamimov's essay investigates the role of Austro-Hungarian aristocratic women who served as nurses in Russia during World War I. Rachamimov compares the work of these nurses, whose main task was to reinforce hierarchy and discipline within the troops, with the more stereotypical Red Cross nurse from Sweden, known as "the Angel of Siberia." Not surprisingly, the soldiers vastly preferred the Swedish nurses who more clearly conformed to gender stereotypes and, what is more important, provided for their needs. The Austro-Hungarian nurses, in contrast, were selected on the basis of their class background and assigned tasks that accorded with their status. What starts out then as a small piece of women's history ends up telling us much more about social relations in wartime Austria-Hungary. Much the same could be said for Maureen Healy's article, which traces the increase in crime and domestic violence immediately following World War I, when defeated soldiers returned to a demoralized Vienna and found themselves facing unemployment, ill health, homelessness, and newly "emancipated" women. Healy's discussion of wartime Austria as a "fatherless" society and the "crisis of masculinity" faced by postwar soldiers clearly indicates how stereotyped gendered images disintegrate in the face of wartime realities. Eliza Ablovatski compares right and left-wing descriptions of violence against women during the revolutions of 1919 to show how both utilized stereotyped images of women within their politics of remembrance.

The second section, "Gendered Collaborating and Resisting," deals with the World War II home front and challenges stereotypical notions of collaboration and resistance. Melissa Feinberg's article examines the activities of the nationalist and right-wing Czech Women's Center and its efforts to tread a fine line between collaboration and maintaining Czech national culture through the promotion of a Czech cookbook. By addressing the nuances of this organization's work, Feinberg points to the irrelevance of such labels as "collaboration" versus "resistance," since neither focuses on motivation, and each excludes some aspect of the group's work, belief, and everyday reality. Feinberg's work dovetails elegantly with that of Benjamin Frommer, which also addresses issues of collaboration. He challenges the assumption that denunciation and fraternization were crimes more frequently associated with women than men. Based also on research in Czechoslovakia, Frommer's essay shows that more men were accused and convicted of crimes of denunciation than women and that while men were just as likely to engage in "horizontal fraternization" with the enemy as women, their acts were less frequently punished since they seemed to bear less serious consequences for the fate of the nation. The third essay in this section also deals with gender policies on the home front in an occupied country during World War II. Mara Lazda compares family policies in Soviet and then Nazi-occupied Latvia. While the Soviet regime promised benefits in social upbringing to benefit both mother and child, brutal deportations that ripped families apart were perceived as a direct attack on the family. The subsequent Nazi occupation forces, in contrast, permitted the reestablishment of some prewar Latvian cultural and social organizations, hoping to use them as a channel for the dissemination of its racist, antisemitic ideology. Although the Nazis intended ultimately to annex and Germanize the region, in the short term they were reluctant to insult or demote the Baltic peoples as their help was still needed in the war. Latvians were able to use this ambiguity in Nazi goals to coopt the family as a sphere where they could exercise autonomy, referring to the ongoing need to strengthen the nation without speci-

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fying which nation they meant. Once again, the line between collaboration and resistance was blurred.

Finally, the third section, "Remembering the War: Gendered Bodies, Gendered Stories," deals with the way in which wartime events have been remembered and memorialized by women both personally and in public. Melissa Bokovoy argues that while women's own experiences in the Balkan Wars and World War I were neglected in public commemorations in interwar Yugoslavia, Serbian women played a central role in creating and maintaining collective memories of the wars in songs and lamentations, as mothers and as the bearers of national memory in charitable and national organizations. Notably absent, however, from the Serbian women's act of remembering were the experiences and sacrifices of the other peoples of the Kingdom of Serbs, Croats and Slovenes, or those who might have suffered at Serbian hands. Bucur's essay also addresses the role of women in the official creation of postwar commemoration. Her research, however, focuses on women's autobiographical writings in Romania following both world wars. Bucur notes that while several women's diaries, memoirs, and postwar oral testimonies were published and valued following World War I, many fewer followed World War II. Even the flood of memoir literature published after the fall of communism has largely excluded women's voices. Katherine Jolluck's essay considers how Polish women remembered their experiences of forced exile in the Soviet Union. She argues convincingly that in this case, as in the past, Polish men and women were able to understand their suffering in national terms. Ultimately, however, the women could not comprehend certain gendered assaults against their persons (such as invasive searches and rapes) as attacks against Poland. After all, they could not be certain that their male counterparts would not see these offenses as having dishonored the nation. Therefore, the gendered attacks had to be internalized, silenced, even forgotten. Lisa Kirschenbaum, too, addresses the key question of which memories were to be made public and heroic and which forgotten. In her essay, Kirschenbaum compares Soviet propaganda that portrayed Leningrad's dead as victims of bombing and its survivors as heroic defenders and the survivors' own memories of a degrading starvation that eradicated visible sex differences and alienated them from their own bodies.

With a very few exceptions the contributions to this volume are solidly researched, coherently organized, and clearly written. In a course I taught on "Women and War" I was surprised to find myself assigning every essay. Even more surprising, I would probably do so again since even the more problematic essays offer sufficiently captivating material to overcome flaws in thesis and structure. I was particularly pleased by the lack of linguistic gobbledygook in most essays. While some may interpret this as evidence of analytic immaturity, I am inclined to view it more positively as simply an appreciation for simplicity and good writing. For that, I suspect the editors also deserve some credit.

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T. Mills Kelly. *Without Remorse: Czech National Socialism in Late-Habsburg Austria*. (East European Monographs, number 689.) Boulder, Colo.: East European Monographs. 2006. Pp. iii, 235. $40.00.

Modern Czech politics was born in the late Habsburg period, a result of progressive expansions of the franchise that culminated in the institution of universal male suffrage for elections to the imperial parliament in 1907. This liberalizing trend undermined the older liberal parties of elites and ushered in the era of mass politics. Among the new parties appearing at this time was the National Socialist Party, described in most accounts as a progressive nationalist party not related to the German party of the same name. While that is technically correct, the party did share some uncomfortable similarities with the later Nazi Party, among them extreme nationalism, the discourse of victimization, and an aggressive style of street politics. Part of a European trend that sought to wean workers away from social democracy with heavy doses of nationalism, the party arose in 1897 after Czech Social Democrats officially renounced Bohemian State Rights, a fundamental principle of Czech politics that demanded a special status in the Habsburg Empire for the lands of the medieval Kingdom of Bohemia. T. Mills Kelly's well-written account of the party's early years is solidly grounded in archival sources and sheds much-needed light on an organization that became an enduring part of the Czech political landscape.

The volume opens with an overview of the social and economic changes of the nineteenth century that made the Bohemian crownlands into the industrial heartland of the empire, followed by an account of the complex political developments that came after the failed revolutions of 1848. Despite ongoing liberalization, the political modernization of the Austrian part of the monarchy was uneven. The curial voting system remained in place for local and provincial elections until the end of the empire, forcing newer parties like the National Socialists to turn to extraparty venues such as newspapers, youth clubs, and adult education programs to propagate their agenda. True to its name, the party attempted to combine nationalism with socialism, incorporating socialist elements like the demand for full and equal suffrage for all, including women, but rejecting the class struggle and internationalism of Marxist doctrine. For them, the nation's enemies were the Bohemian Germans and their "German Jewish" allies, a danger insufficiently understood by the elitist liberals who had traditionally dominated Czech politics. While the National Socialist Party was more outspokenly antisemitic than other Czech parties, Kelly contends that this was only a "rhetorical strategy" since the party ac-

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cepted "Czech Jews" (those who had assimilated to Czech culture) and officially renounced antisemitism in 1902 (p. 62).

If the party's program "offered little new" and even its structure was "imitative of the party's competitors," its confrontational style represented a new direction in Czech politics (p. 69). These tactics are the subject of chapter six, "Taking It to the Streets," which is largely drawn from an article by the same name that the author published in the 1998 volume of *Austrian History Yearbook*. Using a somewhat controversial ecological inference model, Kelly analyzes the Czech electorate in the 1907 and 1911 elections to the imperial parliament, the first based on universal male suffrage. Contending that this is the "first comprehensive attempt to analyze the connection between social status and local economic change and voting behavior in an Austrian region," he compares his voting analysis to a quality of life index to show that the National Socialist Party was most successful in areas where economic growth had stalled relative to other regions (p. 112). On the eve of World War I, with both the Bohemian Diet and imperial parliament suspended, Czech politicians carried on an increasingly bitter struggle outside the political arena, especially in the courts. The spectacular trial of a prominent National Socialist who had been unmasked as a police spy was characteristic of the times. After the war, a much-chastened party emerged under the leadership of the moderate Edvard Beneš, a close associate of the country's president-liberator, Tomáš Garrigue Masaryk, to play a stabilizing role in the new Czechoslovak state.

Although this book is the first to subject the radical fringe of Czech politics to critical analysis, it will be of limited value to those outside the field of Czech studies. It lacks an index, leaves key terms untranslated or unexplained, contains errors in German and Czech, and does not incorporate the statistical tables into the narrative. Although acknowledging that the National Socialist Party was representative of trends elsewhere in Europe, Kelly does not place it in a broader comparative framework. It would have been instructive to see how this party differed from similar movements among other groups in the empire, especially those of its enemy, the Bohemian Germans. Despite these flaws, the book is likely to become a standard work, and a necessary starting point for studies of modern politics and radical nationalism in Habsburg Central Europe.

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SABRINA P. RAMET. *The Three Yugoslavias: State-Building and Legitimation, 1918–2005*. Bloomington: Indiana University Press, with the Woodrow Wilson Center Press, Washington, D.C. 2006. Pp. xxvi, 817. $65.00.

Sabrina P. Ramet is undoubtedly the most prolific scholar of the former Yugoslavia writing in English. The title under review is her ninth book; she has also edited and co-edited nearly twenty other volumes, not to mention numerous journal articles and book chapters. Ramet is a political scientist primarily interested in the countries that once made up Yugoslavia, but she regularly ventures into other disciplines such as gender studies, culture, religion, philosophy, and history. The book under review appeared only a year after her controversial *Thinking about Yugoslavia: Scholarly Debates about the Yugoslav Breakup and the Wars in Bosnia and Kosovo* (2005), in which she assessed practically every book dealing with the break-up of Yugoslavia, dismissing scholars who do not share her interpretations as inadequate at best and morally relativistic at worst. The current volume is too long and not well written; it is unevenly researched and there are some methodological problems; and there are numerous factual errors and typos.

The three Yugoslavias from the title are the interwar kingdom (1918–1941), the Cold War–era socialist republic (1945–1991), and the post-Yugoslav Serbo-Montenegrin federation (1992–2003). The last of the three states was "Yugoslavia" (land of South Slavs) in name only, yet its short history, together with sections dealing with the other post-Yugoslav republics over the past fifteen years, receives far more space than the "first" Yugoslavia (215 pages as opposed to 76 pages; there are 40 pages on World War II and 216 pages on the "second" Yugoslavia). The book's central argument could be summarized as follows: the state Yugoslavs created on three occasions failed because each time it lacked legitimacy. Ramet, however, does not explain why, if this was the case, a united Yugoslavia survived throughout much of the twentieth century and was created and recreated by the Yugoslavs themselves. Her other central thesis—that rather than look into a distant past and/or supposedly incompatible national ideologies for the roots of Yugoslavia's problems, scholars should pay more attention to human agency post-1918—is generally more convincing.

Good points and sensible analysis are often lost in a vast, and clumsily written narrative. Sometimes, they are also overshadowed by the author's eccentric interpretations. Thus, Ante Starčević, a nineteenth-century politician who founded the Croatian Party of [State] Rights, is described as a champion of "a state of equal citizens" who was not interested in "constructing an exclusivist ideology on the basis of either national or religious homogeneity" (p. 39). Despite being in his youth in favor of Serb-Croat cooperation—a position to which some scholars believe he reverted in the twilight of his life—Starčević is known to students of Yugoslavia as someone who spent the best years of his political career arguing that Serbs were "racially" inferior to Croats and/or that they were "Orthodox Croats," and who provided the intellectual foundation for the ideology of the Ustaše, Croatian fascists who ruled the country during World War II. It is of course not Starčević's fault that he was posthumously appropriated by the Ustaše, but if his writings and actions had been really as liberal as Ramet claims, it would hardly have happened. Ramet is right to point out that

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Starčević's follower Josip Frank pursued anti-Serbian policies and was an admirer of racial theories, but her assertion that Frank's Serb equivalent was Orthodox theologian Žarko Gavrilović, active in the 1930s, is puzzling, considering that Gavrilović is at the forefront of religious extremists in present-day Serbia.

Ramet's interpretations are not only often based on errors of fact and in contradiction to the existing scholarship, but sometimes she even contradicts herself. Thus, while the *narodno jedinstvo* and *troimeni narod* ideology is correctly explained as a Yugoslav ideology that provided the basis for the unification of the "trinamed" Serb-Croat-Slovene nation in 1918 (p. 42), Ramet also argues that this ideology was a Greater Serbian program that regarded Slovenes, Croats, and Macedonians as Serbs (p. 234). The feeling that the manuscript was hastily written and not properly proofread is intensified by typos too numerous to list. Ramet appears to struggle even with Yugoslav diacritics, most obviously in chapter eight, where Serbs and Croats are turned into Slovenes, their surnames ending in "č" instead of the Serbo-Croat "ć." A more serious problem concerns Ramet's choice of sources, especially for the "first" Yugoslavia. For instance, why refer several times to a journalist's account of the disintegration of Yugoslavia in the 1990s when writing about the creation of the Yugoslav state in 1918 (p. 38), and not to Ivo Banac's seminal work on the subject (*The National Question in Yugoslavia: Origins, History, Politics* [1984])? Similarly, had Ramet relied on Andrew Wachtel's study *Making a Nation, Breaking a Nation: Literature and Cultural Politics in Yugoslavia* (1998) rather than on a 1940 Croatian publication on the position of the Catholic Church in Yugoslavia, she probably would not have concluded that education served the objective of Serbianizing interwar Yugoslav society (p. 95). By contrast, when relying on primary sources, Ramet produces some of the most insightful sections in the book. Nonetheless, her interpretation of the interwar kingdom does not do justice to the full complexity of Serb-Croat and Serb-Serb relations, while the World War II chapter is similarly simplistic. Sections on later decades of the "second" Yugoslavia and those on more recent events are generally better researched and better argued.

In conclusion, it is not clear for whom this book is intended. Those with little or no prior knowledge of the subject are likely to stay away from such a huge tome and will be well advised to turn to John Lampe's *Yugoslavia as History: Twice There Was a Country* (1996) for a scholarly, balanced history of Yugoslavia. Most specialists will find Ramet's book unreliable, ineptly argued, and overall frustrating. It is a pity that it was not more carefully researched and condensed to a significantly shorter text. A readable, scholarly reassessment of Yugoslavia would be welcome.

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Nathalie Clayer. *Aux origins du nationalisme albanais: La naissance d'une nation majoritairement musulmane en Europe*. Paris: Karthala. 2007. Pp. 794.

George W. Gawrych. *The Crescent and the Eagle: Ottoman Rule, Islam and the Albanians, 1874–1913*. (Library of Ottoman Studies.) New York: I. B. Tauris. 2006. Pp. xi, 260. $74.95.

Although war and political intrigue have inspired intermittent waves of scholarly interest in the Balkans, histories of "exotic" subjects like Albania remain rare. For this reason alone the books reviewed here are a welcome addition to Balkan historiography. Fortunately, these works do more than fill a void. They actually expand our understanding of late Ottoman/Austro-Hungarian history, especially the effect nationalist movements had on both states, revealing the comparative value of studying Albanians to non-specialists.

In order to fully appreciate their comparative potential, these books should be read as a complementary set that link the "long" nineteenth-century Albanian Balkans to current interests in nationalism, diplomatic history, modern state formation, and diaspora studies. Read side by side, the strengths of one compensate for the weaknesses of the other. Nathalie Clayer's work, for example, inspects a massive number of Albanian-language newspapers in order to reveal a multiplicity of Albanian identity forms emerging within the Ottoman diaspora. Clayer argues that all the activisms within these communities are contentious, reflecting still misunderstood regional dynamics and intra-group rivalries that cannot be ignored with conventional references to "tribal loyalties" or "sectarianism" (as is too often done in George W. Gawrych's book). Be it the impact of Hellenism in the 1850s, overtures from Austro-Hungarian diplomats, Albanian-Italian republicanism, and/or anti-Greek/Slav mobilizations, Clayer persuasively argues that we are looking at clashing, multiple processes of identity-formation rather than a kind of primordial ethno-nationalism.

Gawrych, using his close reading of Ottoman-language primary sources, does not consider the impact that the variability among Albanians Clayer discusses may have on the Balkans. While Gawrych recognizes a north/south regionalism among Albanians (he prefers to use the Ottoman terms, *Gegalık* and *Toskalık*), he fails to consider how that single point of distinction may have led to competing interests. Rather, Gawrych identifies a process by which Sultan Abdülhamid II's administration (1876–1909) established a form of "Albanianism" that he suggests accounts for the emergence of an Albanian nationalist identity. The problem is that this cannot account for the possibility that there were, as Clayer asserts, in fact several competing threads of "Albanianism." The multiplicity found in a wide range of non-Ottoman sources not only tells an Ottoman or Albanian story but, according to Clayer, contributes to how we can study nationalism, a conscious effort to make these Albanian cases theoretically relevant to a larger audience that is missing in Gawrych's book.

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It is perhaps this concern with how we theorize the events in the late Ottoman Balkans that distinguishes Clayer from Gawrych most. Throughout 700 information-packed pages, Clayer makes an especially well documented argument about variables of transition that predate by at least a decade Gawrych's own intervening moment of 1874. By failing to consult Albanian-language sources, especially the newspapers produced throughout the 1860s and after the 1896 publishing boom, Gawrych misses the strong fault lines evident within the geographically dispersed Albanian communities. As Albanian historian Hasan Kaleshi first observed in the 1970s and Clayer elaborates here, there are strong connections between how late Tanzimat reformers from the 1850s formally reconfigured Albanians as a self-identified group within the Ottoman empire and the activism Gawrych monitors in considerable detail during, for instance, the 1878–1881 crisis in Kosova. As Clayer demonstrates, the early attempts to create an Albanian *millet* were part of an Ottoman government scheme, one that sought to standardize the language (schools in the Western Balkans would already begin to use Albanian as a medium of instruction by 1856) as well as reconfigure the region into one mega province. This "unified" population would theoretically serve to counter Hellenism and Pan-Slavism infiltrating the Ottoman Balkans after the Crimean War, a logic Ottoman loyalists would repeat after the 1878 Berlin Congress. Here is where Clayer's research proves especially valuable and complements Gawrych's own discovery in the Ottoman documentation that (some) Albanians evoked the need to use the Ottoman state to redraw provincial boundaries and thus protect them from Greek or Slavic expansion. In the end, Gawrych misreads these "unionist" proposals by failing to identify their Tanzimat origins, thus projecting a "nationalist" agenda onto an already well-established set of Ottoman-era reforms that at best cultivated the idea of creating a distinct *millet* in order to stem the influence of outside forces vying for Albanian loyalties.

Clayer's study of this process highlights the need to invest more time into how theorists understood various cases of ethno-nationalism. Clayer's reading of the post-1896 period, for example, indicates an emerging middle class whose idiosyncratic interests cannot be addressed by the Ottoman state policies on which Gawrych's work is reliant. This factor of class anxieties extends beyond the recognized regional (north/south) divides ultimately poisoning the relations between activists in larger Ottoman cities and in the disapora based in Romania, Bulgaria, Italy, Greece, the United States, and Egypt. Indeed, as a result of these rivalries, Albanian factions could not agree on a common alphabet in which to develop their literature, and some even drifted into the dangerous sectarianism found elsewhere in the Ottoman Empire. Ultimately, the factionalism that exploded in Kosova and throughout the southern (Tosk) regions of Ottoman Albania after 1896 resulted in the kind of fragmentation that undermined Albanians' ability to defend their homeland from occupation during the 1912–1913 Balkan wars.

Another fascinating aspect of both studies is the methods by which the state tried to adjust to growing pressures emanating from its multi-ethnic population. Gawrych's close reading of Ottoman documents exposes a policy by the sultan that resorted to nonviolent methods of governance in the hope of placating local demands. The use of state funds to grant jobs to pliant locals, the dissemination of official propaganda, and, according to Gawrych, the efforts to exploit Islamic solidarity all help reinforce a well-composed, if somewhat theoretically vacant, analysis of Ottoman state motivations in the Balkans that challenges unsympathetic views of the regime in past scholarship.

In Gawrych's terms, state officials were more often than not playing the role of "crisis managers" by evoking strong common "Islamic" values or, as he understands them, exploiting interethnic animosities between Albanians and their Greek or Slav neighbors. Perhaps misleadingly, this suggests Albanians' historical role in the late Ottoman period was limited to either being state loyalists or rebellious nationalists. Clayer's analysis suggests that the multiplicity of identity constructions evident in newspapers reveal how regional rivalries and social upheavals, irregardless of "ethnicity," "tribe," or "religion," permitted local actors to engage the larger world in a number of often "contradictory" ways. The range of possible identity claims among Ottoman Albanians—local, sectarian, ethnic, imperial—is thus the major contributing force to how people understood the contingencies interfacing their lives. It is in this varied context that some Albanians (but not all) would resort to using ambiguous Ottoman-era terminology (festishized by Gawrych as unambiguous signs of loyalty or ethnic difference) to respond to regional threats. Where Gawrych sees unequivocal nationalism in this discourse printed in Ottoman (he does not read the vernacular sources), Clayer believes Albanian activism found in newspapers and letters to European consuls are complex and dynamic enterprises that need other kinds of analytical tools to fully deconstruct.

Taking all of this into account, it is somewhat disappointing neither historian engages younger colleagues who, through Michel Foucault and the Subaltern Studies Group (and their critics), have successfully explored the consequences of the emerging modern state on Ottoman Arab societies. This scholarly trend, starting with the work of Eugene Rogan and Selim Deringil and further developed by specialists on Syria/Lebanon such as Ussama Makdisi, Jens Hanssen, and Keith Watenpaugh, has provided the most innovative and interesting scholarship on the Middle East over the last decade. These historians reconsider the impact that state reforms and social and economic integration with Europe had on Ottoman Arabs in Beirut and Aleppo, an angle of exchange that clearly affected the many Albanian responses to change in the Balkans. In the end, that neither Clayer nor Gawrych seems aware of this

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innovative scholarship undermines important comparative opportunities in their work.

Despite this, both books provide the most comprehensive scholarship on late Ottoman Albania to date and should be referenced by future historians as invaluable resources. This especially applies to Clayer's survey of otherwise scattered Albanian-language newspapers. For his part, Gawrych's study offers direct access to the operational and, at times, intellectual dynamics shaping Ottoman Albanian affairs after 1878 as they appear in Ottoman documents and published books, poetry, and plays—resources Clayer never consulted. Although these books together offer much that is new, there is, nevertheless, another book to write. The latter will need to replicate Clayer's vigor and sophistication while matching, if not expanding on, Gawrych's heavy empiricism and at the same time consciously aiming for higher goals. These goals should include making the Ottoman Balkans relevant to the outside world by better integrating the scholarship of other Ottomanists and finally insist that others rewrite Serbian, Greek, Bulgarian, and Turkish modern history in a similar fashion.

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David L. Ransel and Bozena Shallcross. *Polish Encounters, Russian Identity*. (Indiana-Michigan Series in Russian and East European Studies.) Bloomington: Indiana University Press. 2005. Pp. vii, 218. $22.95.

This book is a collection of articles based on papers presented at the conference "Polonophilia and Polonophobia of the Russians" that took place at Indiana University, Bloomington, in 2000. The introductory review essays by David L. Ransel and Bozena Shallcross on Russian identity and its encounters with Poland, and by Barbara Skinner on Russian policies toward the Catholic and Uniate churches, are rather disappointing: they fail to address the most problematic issues and present a rather linear picture of the development of Russian attitudes and policies. For example, Skinner describes the Russian attitude toward Catholicism as one of continuously increasing suspicion and hostility, while presenting the Russian strategy toward the Uniate church as an invariable policy of forced conversion. In reality, the dogmatic anti-Catholicism of the Muscovy period was significantly weakened in the eighteenth century, while the aggressive political anti-Catholicism only began after the Polish insurrection of 1863 and remained selective by focusing exclusively on the Polish nobility. The Uniates' conversion to Orthodoxy in the White Russian lands in the 1830s was a gradual process in which brutal force did not play a dominant role, in contrast to the conversion to Orthodoxy of the Kholm (Chelm) Uniates in the 1870s. The political content of Greek Catholicism was also changing. For instance, the Vatican and Vienna had to make considerable effort in the 1880s in order to decrease the pro-Russian and anti-Polish orientation among Uniates in Habsburg Galicia.

The "Russian Identity" of the book's title ought to have been changed to "Russian Identities": the overwhelming majority of the articles are devoted to analysis of individual experiences of Russian encounters with Poles and Polishness. Some of them bear all the hallmarks of conference papers. Such are the pieces on Mikhail Glinka's appropriation of Polish motives in his music (by Halina Goldberg), on Fyodor Dostoevsky and his Polish comrades in exile (by Nina Perlina), on Vladimir Solov'ev's views on the Polish question (by Manon de Courten), and on the Polish ties of Joseph Brodsky (by Irena Grudzinska-Gross). Other articles—Beth Holmgren's piece on the Polish impressions of the renowned cavalry woman Nadezhda Durova, Megan Dixon's analysis of Alexander Pushkin's poems written in response to the Polish insurrection and its European supporters, and Judith Deutsch Kornblatt's essay on the Polish encounters of Isaac Babel as reflected in his famous *Red Cavalry*—are rich in detail and full of historical findings.

Matthew D. Pauly's article on polonophobic influences on the Soviet policies in Ukraine in the interwar period, which the author obviously has not revisited since 2000, looks like an anachronism after Terry Martin's *The Affirmative Action Empire* was published in 2001.

I will make a special mention of the three articles that constitute a valuable contribution to this rich subject with a long history of study. Leonid Gorizontov's work on the geopolitical aspect of Russian perceptions of their relations with Poles in the nineteenth century and on the imagined geography of the Russian and Polish spheres of cultural and political influence demonstrates the way this competition was connected to the struggle to form different versions of identity and loyalty among the Ukrainian, White Russian, and Polish populations of the empire. Here a tight interweaving of the imperial logic of domination in the Western borderlands and the logic of a Russian nationalism attempted to establish the perception of the Little Russian and White Russian lands as part of the territory of the Russian nationalizing core, not just imperial peripheries. Gorizontov's essay contains a wealth of archival materials and interesting detail. Its most vulnerable spot is the bibliographical references, which too often fail to mention previous contributions to the topic.

Robert L. Przygrodzki tells a fascinating story of the reconstruction of the neo-classic Staszic Palace in the center of Warsaw, initially the residence of the Polish Society of Friends of Sciences, which was remade into a neo-Muscovite style school for Russian boys in the early 1890s. The reconstruction was ideologically linked to the memory of the imprisonment of the Muscovite Tsar Vasilii Shuiskii in the convent located in that place in the seventeenth century. Przygrodzki analyzes the complex symbolism of the new building and shows the interactions of the local community of Russians in Warsaw with the imperial authorities in order to launch the project and to secure funding. This essay is a valuable addition to the research on the imperial policy in architecture that was so actively promoted in the imperial

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periphery during the reign of the last two Russian tsars, Alexander III and Nicholas II.

Andrzej Walicki uses his tremendous expertise in the study of slavophiles to demonstrate the reaction of the slavophile thinkers on the Polish uprising of 1863. He shows, among other things, how, at precisely this time, Ivan Aksakov formulated his thesis of the unbreakable bond between the Polish idea and Catholicism as well as of the necessity to use the "Polish challenge" for the Russian national mobilization. Unfortunately, the book as a whole only states but does not examine the subject of the role of the Polish insurrections. It does not contain essays that analyze the construction and propaganda of political polonophobia in Russia as a phenomenon characteristic of the public mood in the second half of the nineteenth century. The Russian nationalist journalist Mikhail Katkov, who formulated the basic ideas of political polonophobia during the 1863 uprising and managed successfully to implant them in the consciousness of the increasing ranks of his readers, thereby making the Polish subject into a powerful instrument of nationalist agitation, receives (not counting Walicki's essay), only five incidental mentions. Markedly absent is an essay on the political reaction to the 1830–1831 insurrection, and on the role of the uprising in the development of the program of Nicholas I's minister of education S. S. Uvarov. Equally important is the effect of the events of 1830–1831 on the historian N. G. Ustrialov, whose construction, in the 1830s, of the Russian historical master narrative claimed the lands of the Grand Duchy of Lithuania as a Russian national territory. This narrative of the triune Russian nation, comprising the Great, Little, and White Russians, was shaped in a direct opposition to the Polish claim that the eastern territories (*kresy wschodnie*) of the partitioned Polish Commonwealth were an inseparable part of Poland, and turned out to be the most stable element of Russian historiography of all kinds until the early twentieth century, from D. I. Ilovaiskii to V. O. Kliuchevskii.

This book can hardly be called a comprehensive study of the subject; however, some essays in the volume constitute a valuable contribution to the vast and still growing field, and should attract scholarly attention.

ALEXEI MILLER
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D. P. KONDRATENKO. *Samoderzhavie, liberaly i natsional'nyi vopros v Rossii v kontse xix–nachale xx veka* [Autocracy, Liberals, and the National Question in Russia at the End of the Nineteenth Century and the Beginning of the Twentieth Century]. Kirov: Viatskii Gosudarstvennyi Gumanitarnyi Universitet. 2005. Pp. 238.

The liberals of the late tsarist era were outmaneuvered by the autocracy during 1905, outflanked again during the Duma years, caught off guard in the explosive tumult of 1917, and ultimately destroyed by the Bolsheviks during the Civil War. Given this track record, it is perhaps not surprising that their political reputation is fairly dim. Yet the question of *why* the late tsarist liberals were so politically ineffective is, to a degree at least, still open. Were they badly organized or poorly led? Was it their timing or their political agenda? Or does the issue lie less with the liberals themselves than with their opponents? D. P. Kondratenko's useful study argues that liberal ideas, at least on the "national question," were sensible enough. Most liberals, in the center as well as in the borderlands, supported a vision of national cultural autonomy for Russia's peoples within a refurbished, democratically organized empire. As Kondratenko sees it, the reason that the liberals failed so dismally had more to do with the context they faced than with their program.

Kondratenko's book consists of five chapters. The first two are broad overviews of tsarist policies on the "national question" and of the various national movements of the late imperial period. Chapters three and four examine the evolving platforms of liberal parties at the center and on the periphery in regard to the question of national self-determination. The final chapter takes up the issue of liberal politics on the concrete problems of language, schooling, and religious toleration. The general landscape that Kondratenko describes is fairly familiar. To the right stood the tsarist government and its conservative allies with their vision of a more unitary, national state and their mostly hamhanded and overbearing policies of "accelerated incorporation" (pp. 63, 213–214). To the left were the social democrats as well as nationalist separatist parties in the borderlands who supported national rights to the point of outright secession from the empire. And in the middle were the liberals whose position was, appropriately enough, more middle of the road. As Kondratenko shows, with the exception of the Polish National Democrats and the Lithuanian Democratic Party, the various liberal parties of the center and the periphery tended to agree that the best destination for the empire's peoples was not stand-alone national independence but rather increased national cultural autonomy within a Russian-led empire. And the game, as they saw it, was for the long term. The liberals' first objective was democracy. Once democracy was achieved, national cultures within the empire would be respected and the "national question" would be largely resolved.

This noble dream was thwarted on the one hand by the liberals' opponents and on the other by World War I. Kondratenko acknowledges that liberals, by and large, failed to appreciate the full urgency of national resentments in the period between 1905 and 1914. This led them into the complicated and ultimately easily assailable position of criticizing the government's methods to produce greater imperial integration while at the same time supporting the overall objective. Kondratenko shows that, despite the preponderant influence of what he calls "Kadet-Octobrist thinking" (p. 218), none of the various liberal parties ever agreed on a single comprehensive program on matters of religion and linguistic politics. But the real undoing of the liberals was the fact that the middle ground they wanted

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to stand on kept eroding beneath them. Their adversaries on the right and left were too dogmatic to compromise, and war made the gentler approaches of gradualism and moderation all the more impracticable. In the end, time accelerated, events telescoped, and the liberal vision of a united, multinational empire resurrected through democratic reform and long-term national cultural development found itself without either a constituency or a country.

The central contribution of Kondratenko's book is his analysis of the liberal parties of the borderlands, from Poland to Turkestan. By bringing these parties and platforms into view, his work helps to expand our picture of Duma-era liberalism as a political movement that was defined not so much by the personalities and ideas of the center (the way it is usually described) but rather by the creative tensions that unfolded *between* the center and the empire's various peripheries. The book is not the last word on the subject. Kondratenko's treatment of politics is relatively narrow; he is concerned with party debates and parliamentary initiatives rather than with the articulation of political power in broader terms. He also does not really explain how the liberal vision of a refurbished, democratic empire could have come about in the early twentieth century, given the appeal at the time of nationalist and authoritarian solutions. But his study nonetheless provides a useful overview of a major problem of modern Russian history.

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MIDDLE EAST AND NORTHERN AFRICA

MICHAEL AXWORTHY. *The Sword of Persia: Nader Shah, from Tribal Warrior to Conquering Tyrant*. New York: I. B. Tauris. 2006. Pp. xix, 348. $45.00.

The last study on Nader Shah in English, long out of print and unavailable, was Laurence Lockhart's *Nadir Shah: A Critical Study Based Mainly upon Contemporary Sources* (1938). Its range and detail made it an indispensable source on the history of Persia between the fall of the Safavid dynasty and the rise of the Qajars. Since then a number of works in Russian, Persian, and English, including my own translations of two important Armenian sources, have added more details on specific aspects of Nader's reign and personality. Michael Axworthy has not only thoroughly examined these sources but has also utilized two new studies on Nader: Willem Floor's forthcoming *Nader Shah: God's Wrath and Punishment*, and a rare and little-known study by the Greek merchant and traveler, Basile Vatatzes, *Persica: Histoire de Chah-Nadir* (1939).

Axworthy's account is both scholarly and highly readable from start to finish. Unlike the Safavids, Nader and his short-lived Afshar dynasty have, until recently, been generally neglected. This new study argues correctly that without Nader, Persia—or modern-day Iran—would have been partitioned among the Ottomans, Russians, and Afghans. Nader's military campaigns not only restored the borders of Safavid Persia at its height but extended them to Delhi and deep into Central Asia. His use of artillery and his offensive strategies have led some historians to call him the Napoleon of the East. The fear inspired by his armies was so great that during the fifty years following his death (1747), there was no major threat against Persia by its neighbors. It is the author's contention that if Nader had been able to consolidate his military successes, and had passed on a strong state to his sons, his dynasty not only might have preserved Iran's territorial integrity but might also have averted the relative decline of the Muslim world vis-à-vis the West (p. xviii).

Axworthy's account begins with the fall of the Safavid dynasty. He manages to condense the large volume on that subject written by Lockhart in 1938 into a single chapter and corrects some of the errors of the latter. He then proceeds to detail Nader's shrewd political acumen and his rise from a commander in 1720 to shah in 1736. His campaigns against the Afghans and Ottomans and his diplomatic maneuvers with Russia are highly readable, and they reflect an understanding of the political conditions of the entire region. Nader's campaigns in Afghanistan and India get a more thorough investigation than they do in other accounts. Axworthy compares Nader with Timur (Tamerlane), who also invaded India, and who went from tribal warrior to a conquering tyrant who erected towers of skulls.

Axworthy cites every fact or anecdote and explains the various Persian terms mentioned in his sources. The book is thus valuable for both specialists and graduate students of history. The transliteration fortunately follows the modern Persian pronunciation of a word rather than the awkward Arabic version. The lack of diacritical marks and the placement of notes at the end of the book, rather than at the foot of the page, may be criticized by some, but these choices make for easy, flowing reading.

The history of the Iranian world (Eastern Anatolia, the Caucasus and Transcaucasia, and Central Asia) in the eighteenth century has generally been ignored. A number of primary sources in the archives of Armenia, Georgia, Russia, and Turkey are yet to be fully examined. Axworthy's current study on the most crucial period of the history of that region, as well as the life and actions of the most important individual of that era, is a welcome change. Although the author admits that there probably remains untapped material in the abovementioned archives, this new volume presents a wealth of information hitherto unknown and offers some fresh and unique interpretations on the rise and reign of Nader Shah Afshar. Axworthy's book, which is accompanied by illustrations and maps, numerous notes, and a solid bibliography, is highly recommended for all students of Transcaucasian, Iranian, Ottoman, Afghan, and Indian history. I look forward to future works by this accomplished scholar.

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ABDESLAM M. MAGHRAOUI. *Liberalism without Democracy: Nationhood and Citizenship in Egypt, 1922–1936.* (Politics, History, and Culture.) Durham, N.C.: Duke University Press. 2006. Pp. xx, 192. Cloth $74.95, paper $21.95.

In 1996, Dane Kennedy noted that postcolonial theory's major contribution to scholarship has been its "premise that the categories of identity that gave meaning to colonizers and colonized alike cannot be taken for granted: they must be problematized and presented in the context of power." At the same time, he warned that the efforts of many postcolonial purists were "directed against an historical mode of understanding altogether" ("Imperial History and Postcolonial Theory," *Journal of Imperial and Commonwealth History* 24 [1996]: 358, 351). Abdeslam M. Maghraoui's brief monograph illustrates the pitfalls of postcolonial theory. It lacks historical understanding and fails to present the context of power in which he discusses his categories of identity. As the author notes, most of the text was written as a doctoral dissertation in politics fifteen years ago. Inspired in 2003 by the American invasion of Iraq and its stated goal of "engineering democracy through occupation" (p. x), Maghraoui revisited his dissertation to draw parallels between the Bush administration's regime change policy and two occupations of Egypt, that by Napoleon Bonaparte's army in 1798 and that by Britain in 1882. His goal was to establish the link between colonialism and liberalism to explain the failure of Egypt's democratic experiment from 1922 to 1936.

Although this book is aimed at political scientists, Maghraoui never defines the terms "liberals" and "liberalism." Nor does he specify what constituted British colonial liberalism as applied to Egypt. Rather, he argues that Egyptian liberal intellectuals, by calling for parliamentary government and secular reforms, deliberately abandoned their Egyptian identity, rooted "culturally, racially and geographically" in the "Orient," and transferred that identity to Europe. Thus, these Egyptian liberal nationalists, notably Ali Abd al-Raziq, Taha Husayn, and Muhammad Husayn Haykal, "reproduced the same hierarchies of exclusion that liberal colonialism smuggled in through its language of emancipation and progress" (p. 7).

Indebted to Jacques Lacan more than to Edward W. Said or Homi K. Bhabha, Maghraoui presents the language of the Egyptian liberal nationalists as reflecting Lacan's Oedipal formulation that "Desire is the desire of the Other" (p. 35): that is, these nationalists embraced the language of colonial liberalism that sought to assimilate rather than liberate. They strove, consciously or not, to colonize Egypt on behalf of the occupier, Britain, rather than to liberate Egypt from British occupation—despite their claims to the contrary.

Maghraoui's thesis, not clearly stated in the text, has merit, not least because it mirrors in many ways themes found in the writings of Hasan al-Banna and other Muslim Brothers who viewed the modernization effort of Egyptian liberals within the contexts of British occupation, secularization, and cultural imperialism. Regrettably, Maghraoui never provides any valid historical or scholarly context for his arguments. He has not consulted much past scholarship on Egypt, let alone that published since the late 1980s. His final chapter on Egyptian socioeconomic development and his concluding remarks on Sayyid Qutb, if offered as an introductory framework, might have established a basis for comparative analysis grounded in historical developments. As it is, he assumes, erroneously, that all who fall under his Egyptian liberal rubric thought alike throughout the period. And because Europeans were the first to excavate Pharaonic tombs and other sites, Egyptian interest in a Pharaonic identity in the 1920s as a basis of nationalism becomes submission to a European colonial identity. At no point does Maghraoui evince any awareness that many Egyptian liberals abandoned open advocacy of Pharaonism and secularization as early as the later 1920s, including Muhammad Husayn Haykal.

Much has been written on this subject, including Nadav Safran's *Egypt in Search of Political Community* (1961), this reviewer's *Islam and the Search for Social Order in Modern Egypt* (1983),and Israel Gershoni and James P. Jankowski's *Redefining the Egyptian Nation, 1930–1945* (1995). Maghraoui frequently refers to Safran in the text. He lists my book in his bibliography, inaccurately, but never cites it and is apparently unaware that I dispute Safran's thesis on the views of Egyptian liberal intellectuals. He does not list the Gershoni/Jankowski work in his bibliography, but it confirms a broad assertion of an Egyptian Islamic identity in the 1930s that Maghraoui's liberal intellectuals appeared to accept; only Taha Husayn, in 1938, identified Egypt with Europe and the Mediterranean basin and openly rejected any identification with the East, however defined.

The author has imposed his own hypotheses on Egyptian history, disregarding much relevant scholarship. Beyond this, unfortunately, is the question of editorial oversight by Duke University Press. Errors abound in the text and in transliteration of Arabic into English. The Khedive Ismail, correctly noted as being forced to abdicate in 1879 (p. 50), was still in power "at the end of the [nineteenth] century" (p. 47). The al-Azhar scholar, Abd al-Rahman al-Jabarti, who responded to the Napoleonic occupation of Egypt, at one point can be found living "in sixteenth-century Cairo" and being compared to (p. 41) the Iraqi Shi'ite Grand Ayatollah Ali Husaini al-Sistani, currently in Najaf, who is never fully identified and whose name is misspelled.

A stimulating article could be derived from this book, but the book itself does no credit to the author or to the prestigious series, Politics, History, and Culture, in which it was published.

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JAMES F. GOODE. *Negotiating for the Past: Archaeology, Nationalism, and Diplomacy in the Middle East, 1919–*

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1941. Austin: University of Texas Press. 2007. Pp. xii, 293. Cloth $55.00, paper $22.95.

Archaeology is the handmaiden of colonialism and nationalism. If one doubts this fact, James F. Goode makes a lucid, substantive, and convincing case. His emphasis is on the countries of Egypt, Turkey, Iraq, and Iran. In each country archaeology began as a foreign affair, largely the concern of the colonial powers of France and England. Competition between colonial powers extended to fueling their flagships of empire: national museums. In the nineteenth century, briefly reviewed in this book, archaeology was a mining operation to retrieve loot and display it in newly founded European museums. At the turn of the twentieth century, systematic excavations introduced a more rigorous methodology, the conservation of artifacts, and the preservation of architecture. An emerging science was complemented by an emergent nationalism. Prior to World War I, archaeology in the Ottoman Empire was entirely in the hands of foreign missions. Following World War I, with the creation of new nation-states, individuals were appointed to head of state archaeological services. In Syria, Iraq, Jordan, Egypt, and Iran, such persons were always Europeans representing the colonial powers. They were responsible for issuing permits for excavation, managing the division of excavated artifacts, and overseeing the new national museums. Between the world wars, Middle Eastern archaeology experienced its golden age: excavations were undertaken of Tutankhamun's tomb (1922), Ur in Iraq (1927), Persepolis in Iran (1932), and Dura Europos in Syria (1936)—and, one might add, Anyang (1928) in China and Mohenjo-daro (1921) in India—and not only a spectacular array of artifacts but entire civilizations were discovered. The very success of Western archaeologists exacerbated relations with local nationalists, however, and following World War II the situation was fundamentally altered. By 1950, national governments exercised almost complete control over their ancient sites and antiquities.

Of particular value is Goode's emphasis on the role played by prominent Middle Eastern archaeologists in wresting archaeology from the hands of colonial powers. The relations between Satiʿ al-Husri (Iraq), Saʿd Zaghul (Egypt), and Halil Ethem (Turkey) and their foreign counterparts were frequently characterized by intrigue and duplicity. Prominent throughout the book is James Henry Breasted, the first director of the Oriental Institute, gifted to the University of Chicago by John D. Rockefeller, Jr. Although the United States was not a colonial power in the Middle East, it played a prominent role in archaeological excavations. By the middle of the 1920s seven of eight expeditions to Egypt were sponsored by American institutions. Within the United States the Oriental Institute, the University of Pennsylvania, and Harvard University were best connected to the international centers of archaeological activity. Charles Breasted, son of James, appears here as the "heavy." Intolerant of Egyptian nationalism and opposed to Egyptian independence, Breasted's attitude and dealings with Egyptian administrators was nothing short of racist. Nevertheless, within an era of nationalist fervor a foreigner could still act responsibly in the interests of a nation state. This is well illustrated by the Frenchman Pierre Lacau, Egypt's director of antiquities (1914–1936). Under Lacau's direction, Egypt was the first Middle Eastern state to prohibit the division and export of antiquities. Predictably, this infuriated Breasted, who thought Lacau "the most destructive influence that ever beset the scientific efforts of the West in the East" (p. 94). Countering the arrogance of Breasted was Harvard's George A. Reisner. He introduced a scrupulously thorough system of excavation methods and recognized that Egypt's past was not one that was owned by foreign powers. In spite of such men, decades were to pass before American or European institutions assumed any responsibility in the training of Middle Eastern nationals as archaeologists.

Goode is to be richly commended for scrutinizing a remarkable array of original unpublished primary sources. He has exhaustively mined documentary sources from virtually every relevant institution within the United States as well as Egypt, France, Great Britain, Iran, and Turkey. Unpublished sources were further expanded by interviews with senior archaeologists recalling past and present.

Awareness that archaeology was handmaiden to colonialism has, in the past decade, given rise to numerous books, many written or edited by archaeologists. To date, however, Goode's historical study is the most informative, well documented, and judicious in addressing the field of archaeology and its relationship to nationalism and politics. Perhaps it is evidence that the most perceptive critique of a discipline does not come from its own practitioners.

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Kimberly Katz. *Jordanian Jerusalem: Holy Places and National Spaces*. Gainesville: University Press of Florida. 2005. Pp xvi, 214. $59.95.

Historians have written extensively about how modern Jordan acquired an identity. Some suggest that Jordan became Jordanian in the last decades of Ottoman rule as a consequence of Ottoman political and economic reforms that began to give definition to the territory east of the River Jordan. Others suggest that a Jordanian identity only began to form after World War I, when the British occupied the territory and imposed an outsider as its ruler, the Hashemite Amir Abdullah. Not only were there no Jordanians before this time (1921), but once a Jordanian identity emerged it was largely influenced by outsiders: the British, the Hashemite family, and the Palestinians.

Kimberly Katz tells the story of how Jordan's Hashemite rulers incorporated Jerusalem into Jordan's national identity after Jordan captured East Jerusalem during the Palestine War of 1948–1949. Her narrative

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ends with Israel's capture of East Jerusalem during the Arab-Israeli War of 1967.

Katz starts off by describing how the Hashemite Kingdom of Jordan emerged from the shadows of its British patrons and the particular institutions of governance that came to characterize the kingdom. One of her most interesting claims is that King Abdullah I already had designs on Jerusalem before he occupied it. She builds her case around Jordan's new currency and postage stamps, whose images portrayed Jerusalem's ancient heritage.

In her core chapters, Katz concentrates on the different (and not always successful) ways King Abdullah I administratively incorporated Jerusalem into Jordan. The official position of "custodian of the holy places," which he decreed in 1950, did not last long. However, efforts to tax the consular corps of Jerusalem and to promote it as the "second capital" of Jordan, after Amman, signaled that official business concerning Jerusalem needed to be conducted through Amman.

Katz's most illuminating contributions appear in chapter five. She illustrates with rich examples how in the 1950s and 1960s King Hussein (Abdullah's grandson) and other leaders defined Jordan as the Holy Land, and the kingdom as the guardian of some of the most revered religious sites for Muslims, Christians, and Jews, mainly but not exclusively in Jerusalem. It was in this period that the government placed special emphasis on the promotion of tourism for Western Christians, and that tourism became an integral part of the Jordanian nation-building project.

Katz describes the evolution of different kinds of tourism. Religious tourism concentrated both on the individual holidays of the different faiths and on large events such as the hajj (pilgrimage) to the Muslim holy sites in Saudi Arabia, which Jordanian officials promoted as a full pilgrimage only when Jerusalem was part of the tour, even though Jerusalem was never "a prescribed part of the hajj" (p. 123).

Pope Paul VI's 1964 visit to the Holy Land (the last papal visit had been St. Peter's) provided the occasion for King Hussein to promote closer relations between Christianity and Islam. The pope's visit was used successfully to increase tourism, and with it new foreign and domestic investment; moreover, it helped to legitimate the idea of Jordan as the Holy Land. The issuance of a new postage stamp series featuring the likenesses of both the pope and King Hussein only reinforced Jordan's claim. Katz's close reading of a variety of texts—especially brochures, medallions, and stamps—enriches her explanation of how the kingdom rendered as "Jordanian" holy sites that had historically been associated with Jerusalem.

Katz concludes with two chapters on the establishment of the Palestine Liberation Organization (PLO) in the mid-1960s and the impact of the 1967 war. The PLO posed the first challenge to Jordan's claim to Jerusalem, a challenge that grew stronger after Jordan ceded control of East Jerusalem and its holy sites to Israel during the war. In its aftermath, the government refocused its identity-building enterprise by promoting tourism to non-religious historical sites in Jordan proper, in particular Petra and Jarash.

This book has two limitations. One is that Katz was unable to gain access to Jordanian government archival sources, owing to the political sensitivity of her topic. They might have reinforced and enriched a number of her interpretations and obliged her to revise or abandon others. The absence of warm bodies in the narrative is the other limitation. Katz tells us little about ordinary Jerusalemites or how nearly two decades of Jordanian rule conditioned their lives and prospects. We learn about the shaping of a Jordanian identity but very little about the shaping of a Palestinian identity. Historians are, of course, free to define their topics as they see fit, and from the outset Katz clearly sets the boundaries of her study. The result is an original but somewhat narrow work of historical scholarship.

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ISABELLA GINOR and GIDEON REMEZ. *Foxbats over Dimona: The Soviets' Nuclear Gamble in the Six Day War.* New Haven, Conn.: Yale University Press. 2007. Pp. xi, 287. $26.00.

Almost twenty-five years ago, Shlomo Aronson and I realized that the nuclear element was a crucial contributor to the escalation that led to the June 1967 Six Day War. This was based on a wealth of Arab expressions of alarm, and threats against Israel's nuclear option, that we had unearthed. The U. S. Ambassador in Cairo, John Badeau, warned President Lyndon B. Johnson that Egypt would launch a pre-emptive war against Israel if it pursued a nuclear weapons capability. The October 1965 Third Arab Summit in Casablanca convened with this brouhaha at the top of its agenda. Egyptian threats to launch a pre-emptive war, issued with increasing frequency from 1960, and on an almost daily basis from 1965, were making Israeli decision makers increasingly nervous and defensive. Still, our findings, based exclusively on open sources, emphasized that it was the threat posed by Egypt that elicited escalatory Israeli reactions that brought the crisis to a head.

The book under review offers a novel, intellectually worthy, and stimulating thesis: it was the Soviet Union that instigated the Arabs to act in this way, or at least fueled their actions, because the Soviet leadership desired the destruction of Israel's nascent nuclear capability and was convinced that an Israeli attack would lead Arab forces to destroy the Dimona reactor. The early chapters of the book describe some of the Soviet intrigue and manipulations of the Arab escalation, but most of the evidence presented does not proceed to prove its premise: the authors gradually and increasingly digress to a multitude of issues irrelevant to the central idea.

The book is heavy on details of Soviet forces' planning, deployments, movements, and intentions to intervene militarily against Israel, based almost exclusively

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on anecdotal evidence from personnel involved. It also details for the first time a couple of minor skirmishes in which Soviet personnel were probably involved during the fighting, heretofore the subject of conjecture and hearsay. Isabella Ginor and Gideon Remez ought to be congratulated for peeling away the layers of Soviet obfuscation and praised for warning the readers that Soviet documentation is unreliable and circumspect. But however well presented by the authors, the extensive Soviet contingency planning does not really come as that much of a surprise. The authors demonstrate, in the last few chapters, how very seriously this possibility was taken by the parties involved.

Ginor and Remez sometimes make mountains out of molehills: an informal chat between two gossipy Israeli has-beens, about Israel's intention to manufacture nuclear weapons, is interpreted as a deliberate "message" delivered to Moscow; if true, it would have been irresponsible, foolish, and potentially suicidal, and therefore makes no sense. This is one example of the authors' occasional unjustifiable lapses into conspiracy theory. In another case, they devote an entire chapter to a document dated five days after it was delivered, as proof of another conspiracy.

The authors present interesting new evidence about the type of aircraft involved in reconnaissance flights over Dimona, as is reflected in the title of the book. But regardless of the type of aircraft, and whether the pilots were Soviet or Egyptian, more important was that the Israeli leadership assumed the flights signaled an impending attack by Egyptian forces—the oft-threatened pre-emptive war—and this led it to take escalatory precautionary measures.

The main contribution of the book is in the detail of the contemplated Soviet military intervention, as told by its participants. It also shows how the Soviet political leadership generally misrepresented its own decisions and actions for posterity. This could have interesting implications for the current context of a Second Cold War. But the main thesis that the authors set out to prove—that the Soviet Union instigated the 1967 War to destroy Israel's nascent nuclear capability—although illuminated and given modest empirical support, requires more substantive evidence. It escapes Ginor and Remez that their elaborate descriptions of Soviet military preparations for intervention fail to support their original stated thesis that the Soviet leadership wanted Arab forces to destroy Israel's nuclear capability. The last few chapters of the book provide a detailed account of Soviet planning for a military intervention, driven by altogether other considerations, which in the end came to naught. The authors' account is commendable for its dramatic representation of the Soviet leadership's bemused grasp of the situation, and its confused decision making, after the first few hours of the war demolished its house-of-cards strategy.

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Uzi Rabi. *The Emergence of States in a Tribal Society: Oman under Saʿid bin Taymur, 1932–1970.* Brighton, England: Sussex Academic Press. 2006. Pp. xii, 299. $75.00.

Uzi Rabi has written a book that depicts the long reign of one Arab ruler in the southern Arabian peninsula and more generally casts light on such matters as the origins of states, the complex interrelationship of tribes and centralizing authorities, and the impact of imperial powers on partially dependent societies. The author concentrates on the personal role, vision, and accomplishments of Sultan Saʿid of Oman as he established his primacy over the tribes of the interior through a careful use of rewards and punishments, while his government enjoyed the crucial support of Britain.

Rabi first describes the flexibility of tribal alliances and identities. He then outlines the history of Oman up to the time when Saʿid came to the throne in 1932. The new sultan chose to cut expenditures, hoping to avoid borrowing from abroad. In the 1940s and 1950s Saʿid slowly chipped away at the independence of the tribal leaders while receiving substantial British military and diplomatic aid so as to increase the size of his armed forces and claim rights to the Buraymi oasis region where oil was expected to be found. After Saʿid gained control of the interior in 1955, both conservative Saudi Arabia and revolutionary Pan-Arab Egypt backed groups in Oman or in exile outside it that were opposed to the sultan and his British advisors. By the end of the 1950s most resistance to the sultan in the remote parts of the country came to an end. He then turned to managing what Rabi terms a Unified Tribal State: that is, a loosely organized central administration that curbed the power of tribal leaders while still preserving tribal identity and local autonomy. The sultan opposed many British proposals for extending the power of the central state at the expense of the tribes, the author argues, not because of any unreasonable aversion to change in and of itself, but rather because he feared that such measures would be too expensive and unpopular, and that they would erode the basis of his patriarchal rule. Nevertheless, Saʿid did approve some changes in fields such as health care while remaining skeptical with regard to others, especially public education.

A series of dramatic developments in the late 1960s threatened Sultan Saʿid's policies of manipulating the tribes and very slow change in society. The issue of how to use the new revenues arising from the exploitation of oil, a regional rebellion in Dhufar backed by neighboring southern Yemen, and the impending withdrawal of British forces from much of Arabia opened the way to the 1970 ouster of Saʿid by his son and heir, Qabus, who has ruled Oman since then. Sultan Qabus and his adherents claimed that they were bringing a completely new age and radical changes to a country where Saʿid had done little or nothing to foster political, economic, and social development.

Using primarily British diplomatic records as well as a wide variety of secondary sources, Rabi makes a convincing and well-argued case that most foreign histo-

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rians uncritically accepted the statements of the new regime, thereby unfairly ignoring the substantial accomplishments of Sultan Sa'id. This revisionist interpretation is carefully nuanced so as to take into account the sultan's eccentricities and mistakes even while presenting a mostly favorable view of his actions and policies.

In addition to this useful contribution to historical scholarship, Rabi applies to the case of Oman the theoretical framework developed by Joseph Kostiner with regard to the formation of the state in Saudi Arabia from 1916 to 1936. Kostiner and Rabi both argue forcefully that managing tribes was crucial to the strengthening of the central authorities, particularly before the full impact of oil revenues provided numerous other tools for rulers to become supreme. The Unified Tribal State was based on personal relationships between rulers and tribal members rather than bureaucratic controls, and it operated in a physical environment whose borders were variable rather than fixed. Even after the central state gained military supremacy over the tribes in Saudi Arabia and Oman, relations with foreign protectors, first Britain and later the United States, were of great value to the rulers of the two countries in maintaining themselves in power. Rabi's book is particularly valuable in its critical analysis of British-Omani relations, showing that while Britain usually supported Sultan Sa'id it also sometimes opposed and criticized him for not following British approaches to state formation, going so far as quietly to support the 1970 coup that ended his reign.

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HAGGAI ERLICH. *Saudi Arabia and Ethiopia: Islam, Christianity, and Politics Entwined*. Boulder, Colo.: Lynne Rienner. 2007. Pp. xi, 249. $49.95.

Saudi Arabia and Ethiopia are important regional powers in the international relations of the Middle East and Africa, but little attention has been given to their relations with each other. Saudi-Ethiopian relations are, however, a major element in shaping power relations in a strategically significant region. Haggai Erlich presents a multidimensional analysis of Saudi-Ethiopian relations since 1930, with special attention given to the impact of changing Muslim and Christian self-understandings on these relations.

Erlich provides broad portraits of two states in which religio-historical legacies provide important foundations for identities of the states themselves. The "revitalization of . . . Ethiopia as a Christian empire" (p. 14) under the leadership of Haile Selassie in 1930 coincided with the similar revitalization of an Islamic revivalist tradition when Abd al-Aziz ibn Saud established the modern Kingdom of Saudi Arabia. The evolving definitions of political self-identity and of the religio-political identity of the other state provide Erlich with the keys to understanding the nature of Saudi-Ethiopian relations. His analysis revolves "around the dialectical interplay between formative religious legacies and concrete issues of international politics" (p. 7).

The first chapter defines the conceptual foundations of the religious legacies. Muslim perspectives start with the story of early Muslims, during the lifetime of the Prophet Muhammad in the seventh century C.E., finding refuge and acceptance in the Christian Kingdom of Ethiopia. Two Muslim narratives shaped later attitudes toward Ethiopia, one emphasizing the need for continuing positive Muslim relations with Ethiopia and a second reporting that the early Christian king had converted to Islam but that the church suppressed Islam, providing the rationale for activist efforts to conquer and convert the Ethiopians.

The new states established by Haile Selassie and ibn Saud are examined in the second chapter. Christian Ethiopian concepts of Islam are placed in their historical context, noting both the strong Middle Eastern orientation of the Ethiopian church resulting from ties to the Coptic Church in Egypt and the fear of the proximity of the Middle East because of what Erlich calls the "Ahmad Gragn trauma" (p. 19). The latter refers to the sixteenth-century wars in which Muslims under the leadership of Ahmad Gragn almost succeeded in bringing an end to the Ethiopian Christian state.

Succeeding chapters provide analyses of the evolving relations between Ethiopia and Saudi Arabia. An early issue involved the expansion of Italian imperialist claims in the Red Sea area. As described in chapter three, Saudi Arabia did not oppose Benito Mussolini's conquest of Ethiopia. In chapter four, fascist policies favoring Ethiopian Muslims provided a basis for good relations between Italy and Saudi Arabia, but during World War II, the Saudis cooperated with the British war effort. In the immediate postwar period, Erlich notes the development of a competition between two different modes of Islamic identity. One represented continuity with the past traditions of Muslim leadership in the major Muslim city of Harar that was "rather a pragmatic mixture of Sunni moderate orthodoxy and Sufism" (p. 83), while the second reflected the influence of a more strict "Wahhabi" perspective in the tradition of the jihad of Ahmad Gragn.

During the third quarter of the twentieth century, as covered in chapter five, both Saudi Arabia and Ethiopia were religiously traditional monarchies at a time when radical revolutionary ideologies were a major concern. Despite some problems with a Muslim-oriented liberation movement in Eritrea, Saudi-Ethiopian relations were relatively undramatic. This situation was transformed with the overthrow of the Ethiopian monarchy by a radical Marxist regime in 1974. As described in chapter six, the era from 1974 until the fall of the Marxist regime in 1991 was a time of "mutual demonization" (p. 133). In this context, "Ethiopia's Muslims remained mostly silent" reflecting "a deepening duality among Muslims" between the older Harar traditions and a new Wahhabi activism. Chapter seven places Saudi-Ethiopian relations in the contexts of the 1990s, with state-to-state relations viewed by Erlich as less important

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than the rise of activist, and sometimes extremist-militant, Muslim groups and new Christian political attitudes in the post-imperial, post-Marxist era.

Erlich's analysis is a comprehensive discussion of "the ever-changing interplay between three cultural systems: the Christian-Ethiopian, the Saudi-Wahhabi, and the Islamic-Ethiopian" (p. 223). However, a dimension of this interplay that could be made more explicit by Erlich is that each of the cultural systems shaped a distinct response to "modernity." Given Erlich's emphasis on the processes of societal self-definition, it would be useful to have a more explicit presentation of how the conditions of modernity affected the nature of the cultural interactions.

Erlich's work bridges the common conceptual gap that separates "African studies" from "Middle Eastern studies." It provides important insights into the religious dimensions of international relations and is an excellent introduction to the new modes of foreign relations that characterize global affairs in the twenty-first century.

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SUB-SAHARAN AFRICA

MARY WREN BIVINS. *Telling Stories, Making Histories: Women, Words, and Islam in Nineteenth-Century Hausaland and the Sokoto Caliphate*. (Social History of Africa.) Portsmouth, N.H.: Heinemann. 2007. Pp. xiii, 192. Cloth $89.95, paper $29.95.

Mary Wren Bivins has undertaken an ambitious task: to reconstruct women's history in the Sokoto Caliphate, a nineteenth-century Islamic state in West Africa. To this end she has carefully and creatively employed an interesting and diverse array of sources, from Arabic documents and travelers' accounts to traditional folktales. She has produced a book of interest not only to scholars of Islamic reform in Africa and elsewhere but also to scholars of women's and social history.

The ordinary women whose lives and thoughts Bivins tries to reconstruct are not directly accessible through documentary sources, of course. Thus this is not and cannot be a history in the traditional sense, a traditional narrative. Rather it is an attempt to reconstruct the largely undocumented situation of Hausa women during the period of Islamic reform in the Sokoto Caliphate. It attempts to assess the social condition of those women, and to see how they understood and reacted to the changes they encountered.

This is not to say that there are no writings by women from the time in question. Several women in the families of the ruling Islamic reformers left voluminous writings in support of the Islamization of their society. Bivins understands that such women were at odds with the traditional women's culture of the area and reads their writings carefully, as responses to that culture, in order to try to understand the way their activities changed that culture. Thus she situates their writings in a discourse of which we have only one half. The outcome of that discourse was modern Hausa society.

In addition to the writings of women of the ruling class, Bivins draws on the writings of European travelers who passed through the area, especially their observations about women and gender relations. Again her approach is careful and judicious, if creative, teasing as much as she can out of these sources. If they are almost entirely male sources, biased and incomplete in their viewpoint, that is not her fault, and she shows that they can be useful for her purpose.

The most problematic source the author uses in her reconstruction of the past is folktales. This is not oral history in the ordinary sense. These tales are not narrated to be taken as factual statements about the past, however problematic any narrative about the past may be. In fact the Hausa themselves often refer to such tales as "lies." Rather these are ordinary folktales out of which Bivins tries to extract the social attitudes of women in the precolonial past of Africa. Whether or not she succeeds is highly problematic.

Traditional folktales are orally improvised, and never told exactly the same way twice. They are performed by their reteller to please an audience. The conditions of the actual telling (and recording) of any particular version of any folktale are of the utmost importance in evaluating the tale for any purpose, especially the reconstruction of ideological history. Most of the texts of Hausa folktales that Bivins uses were written down by Islamic scholars at the behest of colonial officials in the early colonial period and were used by colonial officials to study for the Hausa-language examinations.

Furthermore, the narrative of all folktales is inherently exceptional. They may express the general concerns of the society in which they are told, but no more so than action movies would express the general concerns of modern American society. Both are escapist entertainment, a deliberate counterpoint to daily life, not an explanation of it, or even necessarily a commentary on it. Folktales also travel widely. There is a Hausa version of the Cinderella story, which originated in Tang Dynasty China. Would any particular retelling of that folktale in the Hausa language necessarily give more insight into Hausa society than Chinese? The lack of any use of any tale type or motif index in this work suggests that the author has not familiarized herself with the scholarship pertaining to folktales.

This is an important work considering an important aspect of women's history in Islamic Africa. The weakest point of its analysis is Bivins's use of folktales as sources. This could have been rectified by some serious consideration of what folktales are and how and why they spread, as studied by folklorists.

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Collected Essays

These volumes, recently received in the *AHR* office, do not lend themselves readily to unified reviews; the contents are therefore listed.

COMPARATIVE/WORLD

MARGARET R. GREER, WALTER D. MIGNOLO, and MAUREEN QUILLIGAN, editors. *Rereading the Black Legend: The Discourses of Religious and Racial Difference in the Renaissance Empires*. Chicago: University of Chicago Press. 2007. Pp. vii, 478. Cloth $65.00, paper $25.00.

LESLIE PEIRCE, An Imperial Caste: Inverted Racialization in the Architecture of Ottoman Sovereignty. RUBY LAL, Hierarchies of Age and Gender in the Mughal Construction of Domesticity and Empire. DAVID NIRENBERG, Race and the Middle Ages: The Case of Spain and Its Jews. BARBARA FUCHS, The Spanish Race. IRENE SILVERBLATT, The Black Legend and Global Conspiracies: Spain, the Inquisition, and the Emerging Modern World. GONZALO LAMANA, Of Books, Popes, and *Huacas*; or, The Dilemmas of Being Christian. SILVERMOON and MICHAEL ENNIS, The View of the Empire from the *Altepetl*: Nahua Historical and Global Imagination. YOLANDA FABIOLA ORQUERA, "Race" and "Class" in the Spanish Colonies of America: A Dynamic Social Perception. KATHRYN BURNS, Unfixing Race. CARMEN NOCENTELLI, Discipline and Love: Linschoten and the *Estado da Índia*. PATRICIA GRAVATT, Rereading Theodore de Bry's Black Legend. EDMUND VALENTINE CAMPOS, West of Eden: American Gold, Spanish Greed, and the Discourses of English Imperialism. LINDA BRADLEY SALAMON, Blackening "the Turk" in Roger Ascham's *A Report of Germany* (1553). JEFFREY KNAPP, Nations into Persons. WALTER D. MIGNOLO, What Does the Black Legend Have to Do with Race?

NANCY L. RHODEN, editor. *English Atlantics Revisited: Essays Honouring Professor Ian K. Steele*. Ithaca, N.Y.: McGill-Queen's University Press. 2007. Pp. xxviii, 528. $85.00.

NANCY L. RHODEN, Revisiting *The English Atlantic* in the Context of Atlantic History. RICHARD R. JOHNSON, Changing Spaces: Reconstructing the Political Economy of Empire. JOHN SHY, Ian K. Steele as Military Historian. BARBARA C. MURISON, The Talented Mr Blathwayt: His Empire Revisited. RANDY DUNN, Patronage and Governance in Francis Nicholson's Empire. STACY L. LORENZ, Policy and Patronage: Governor William Gooch and Anglo-Virginia Politics, 1727–1749. NEIL KENNEDY, William Crashaw's Bridge: Bermuda and the Origins of the English Atlantic, 1609–1624. SARA MORRISON, Forests of Masts and Seas of Trees: The English Royal Forests and the Restoration Navy. MICHAEL DOVE, Plying the Northernmost Atlantic Trading Route to the New World: The Hudson's Bay Company and British Seaborne Empire. DANIEL A. BAUGH, The Atlantic of the Rival Navies, 1714–1783. JON W. PARMENTER, "Onenwahatirighsi Sa Gentho Skaghnughtudigh": Reassessing Haudenosaunee Relations with the Albany Commissioners of Indian Affairs, 1723–1755. ALEXANDER V. CAMPBELL, Atlantic Microcosm: The Royal American Regiment, 1755–1772. MICHELLE A. HAMILTON, In the King's Service: Provisioning and Quartering the British Army in the Old Northwest, 1760–1773. DAVID J. NORTON, Samson Occom: Mohegan Leader, Christian Shaman, and Christian Sachem. KENNETH A. LOCKRIDGE, Overcoming Nausea: The Brothers Hesselius and the Great American Mystery. NANCY L. RHODEN, Patriarchal Authority in Revolutionary Virginia: Connecting Familial Relations and Revolutionary Crises. MARGARET M. R. KELLOW, "We Are No Less Friendly to Liberty Than They": British Antislavery Activists Respond to the Crisis in the American Colonies. JOHN M. MURRIN, The English Atlantic Created a Man of Steele Who Reciprocated by Creating It.

JAMES DELBOURGO and NICHOLAS DEW, editors. *Science and Empire in the Atlantic World*. New York: Routledge. 2008. Pp. xiv, 365. $31.95.

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JORGE CAÑIZARES-ESGUERRA and ERIK R. SEEMAN, editors. *The Atlantic in Global History, 1500–2000*. Foreword by THOMAS BENDER. Upper Saddle River, N.J.: Prentice Hall. 2007. Pp. xxviii, 258. $33.60.

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CANADA AND THE UNITED STATES

RICHMOND F. BROWN, editor. *Coastal Encounters: The Transformation of the Gulf South in the Eighteenth Century*. Lincoln: University of Nebraska Press. 2007. Pp. xiii, 313. $24.95.

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FEATURED REVIEWS

To the Editors:

Robert Allen's review of *Ending the French Revolution: Violence, Justice and Repression from the Terror to Napoleon* (*AHR*, December 2007, 1487–1489) requires a response, for despite describing my book as "challenging and controversial," his essay fails to convey why this is so. Even when stating that my argument about the continuities between the Directory and the Consulate contrasts with that held by the "vast majority of historians," he contradictorily adds that two works offer "a similar periodization." In fact, both these works are short surveys that run to 1804, when France became an empire, not 1802, when Napoleon became a dictator for life. Apart from simple accuracy, the larger point is that the strength of an historical interpretation depends on the quality of its evidence and the cogency of its argument, not how many people agree with it or who said it first.

Thence to the argument. As my introduction makes clear, I chose to end my book in 1802, rather than 1799 or 1804, because this is when a stable regime finally emerged. Such a moment was made possible only by mitigating the four forces provoking the greatest instability in French society: Catholicism, war, royalism, and Jacobinism. The histories of the Concordat, the Peace of Amiens, and the defeat of democratic parliamentarism are well known; what my book adds is the history of the tension between justice, violence, and repression in the years after the Terror. At first, this tension helped to prolong revolutionary instability, but later, once exceptional measures became better regulated and a more robust security apparatus took shape, the ability to address sources of violence helped to make the other, better studied, solutions possible. Together these created "liberal authoritarianism" as well as the basis for a personal dictatorship. Even such a cryptic overview—in marked contrast with what could have been said in a featured review—makes it clear that my book is more about how the French Revolution ended (hence the gerund in the title) than when it did so.

Rather than argue with this interpretation, Allen prefers to dwell on those sections of the book which relate directly to his own area of research—the criminal courts of the period. Even then he fails to convey my basic claims about these courts—namely, that their supposedly high acquittal rates were in fact comparable to those of military courts and special tribunals, that these forms of expedited justice proliferated mainly because juries proved reluctant to convict in cases of resisting the republican state, and that the decline in activity of criminal courts after 1800 masks an actual increase in judicial repression brought on by special tribunals. In general, Allen prefers not to engage larger issues raised by the book. Thus, rather than deal with my argument that the Directory began as a valiant—albeit flawed—attempt to establish liberal democracy, Allen contests the novelty of the *Code des délits et des peines* of 1795. Likewise, rather than discuss the usefulness of "security state" as a way to understand the Consulate or whether a society's "economy of violence" is basic to its nature, Allen faults me for language that is "often opaque"; rather than assess my use of particular theorists such as Michel Foucault, Sergio Cotta, Carl Schmitt, and Giorgio Agamben, he dismisses theoretical constructions as reductionist; rather than comment on my use of comparative material from ancient Athens to Tsarist Russia, or my interpretive challenges to major thinkers on the French Revolution, such as Lynn Hunt, François Furet, and Pierre Rosanvallon, Allen criticizes me for not citing select papers in obscure conference proceedings and for simply mentioning flaws in the research of others.

When Allen does raise issues of some importance to my book, he misleads. He presents my explanation of the Fructidor coup d'état as though it were the same as

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the Directory's and claims that it lacks evidence. Such a distortion ignores my extended passage on the matter (pp. 246–247) and the several books on which it is based. He does similar injustice to my analysis of pre-revolutionary France as an "organic society" by portraying my concept as static, stateless, and unsubstantiated. Such treatment is either tendentious or evidence that he really did find my book "a difficult read." I invite others to decide for themselves.

HOWARD G. BROWN
Binghamton University SUNY

ROBERT ALLEN RESPONDS:

Howard Brown raises some viable issues in his letter—points that, as he suggests, can be left to our colleagues to decide. By nature, issues of periodization may be argued *ad infinitum*. Whether the terminal point of the French Revolution is placed at Brumaire, as most scholars have done, or whether the Consulate in whole or in part is collapsed into the Revolution, as other scholars, including Brown himself, prefer, depends partly on which strands of historical development are stressed. This point is certainly not contradictory, and the term "similar periodization" was used in this sense, instead of a notion of "identical" periodization that Brown appears to read into the sentence.

The core of Brown's study (the "how" as distinct from the "when") lies in the history of justice and of the police. *Ending the French Revolution* broadens our knowledge of those institutions. And it is certainly true that "who got there first" is not the ultimate standard for judging scholarship. But the discipline of history imposes on all of us the sometimes vexatious responsibility of "placing" our own work realistically into the existing historiography. It should not be construed as a flight from substantive debate for a reviewer to point out that, while Brown often fulfills this task in extensive footnotes, his omissions—and even, at times, misleading or overdrawn statements—allow readers to think that his work is more novel than it truly is. Stephen Clay's article on the *état de siège* appeared not in an "obscure" conference proceeding but in the publication from a major French conference held to mark the bicentennial of the Directory and entitled *La République directoriale*. Brown cites an article from that volume, but not that of Clay. Concerning articles that do appear in less prominent publications, the realistic "facts of academic life" are well known, but the force of that axiom should be counterbalanced by a recognition that scholars who publish in less prominent journals or collections are placing their findings in the public domain, and deserve some sort of protection from the possibility that their work might be used without acknowledgment.

Though my review did not "dwell" on the criminal courts, I will do so here. Even Brown's letter to the editor makes exaggerated claims for his own work. "His" point that the perceived weakness of the ordinary juries in cases of resistance to the state, including brigandage, helped lead to forms of expeditive justice was made decades ago by Esmein and Cruppi, and more recently by Schnapper, by Moyaux, and in the work of Lascoumes, Poncela, and Lenoël. Nor is the idea that the founding of special tribunals after 1800 brought an increase of repression original to Brown. Of the three "basic claims" for which he takes credit in the third paragraph of his letter, the one that is unequivocally "his" is the first—and this point, I acknowledge, should have been mentioned in my review.

My review, finally, did not suggest that Brown's book treats pre-revolutionary repressive techniques simply as static and stateless. It argued rather that his use of the term "organic" impoverishes the complexity of Old Regime repression and of its sometimes abrupt fits and starts. My argument in the review was with the analytic construct. Even in the book's development, however, Brown emphasizes much more strongly the role of local or popular initiative in Old Regime social control than the forms of state repression that coexisted with them and were quite capable of superseding them (see pp. 67–68, and, for a striking example of the vagaries of state repression, Arlette Farge's *The Vanishing Children of Paris*). It should be recognized that the movement toward more effectively repressive state structures played out over a span of centuries. The revolutionary and Napoleonic periods, as historians have long emphasized, witnessed a great leap forward in that sense, but preceding leaps are only with great difficulty subsumed under the term "organic."

ROBERT ALLEN
Stephen F. Austin State University

TO THE EDITORS:

As the editor of Martin Malia's posthumously published *History's Locomotives: Revolutions and the Making of the Modern World*, I am grateful for Charles Tilly's thoughtful featured review of the book (*AHR*, October 2007, 1120–1122). The review focuses its criticism of the book on methodological issues of comparison and causality, bypassing consideration of the accuracy of its historical narratives. It seems to me that Tilly's remarks on these issues constitute a nice demonstration of what Malia sees as the shortcomings from an historicist point of view of the structural-functional categories of "high social science": essentially, "revolution" occurs any time, any place—given the presence of certain structural and/or functional elements. Tilly tells us that Malia's comparative method fails John Stuart Mill's test for comparisons, which "require[s] that the cases compared be causally independent of each other." Of course it does: Malia follows Tocqueville and Marc Bloch in applying comparative historical inquiry to a cultural-geographic continuum in a search for differences rather than similarities; in essence, a widening of

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the study of historical processes beyond national borders but within a cultural continuum.

TERENCE EMMONS,
EMERITUS
Stanford University

CHARLES TILLY RESPONDS:

Thanks to Terence Emmons for stressing a point my review of Martin Malia's *History's Locomotives* did not make sufficiently clear. Malia and I did indeed disagree about the possibility of systematic analysis of change and variation in the circumstances and character of revolutions.

Nevertheless, my own work on the subject strongly rejects the idea of universal models for revolution; see, for example, my *European Revolutions, 1492–1992*, published in 1993. In any case, Malia's book never resolves the contradiction between insisting on the distinctiveness of each great revolution and the argument that each new revolution occurs, at least in part, as a result of conditions produced by the previous revolution.

CHARLES TILLY
Columbia University

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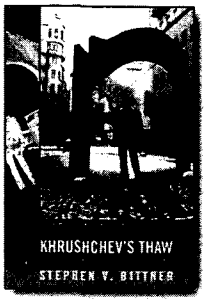
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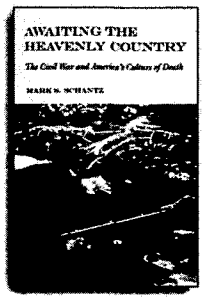


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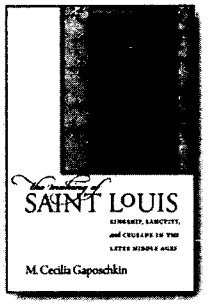
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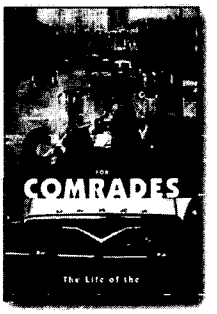


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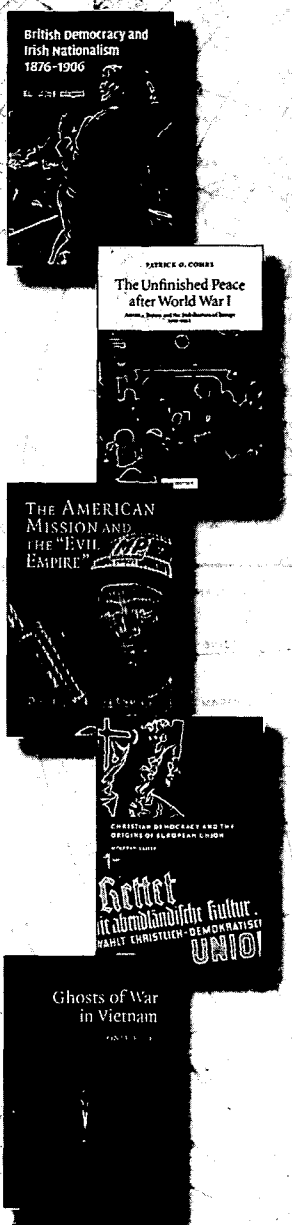
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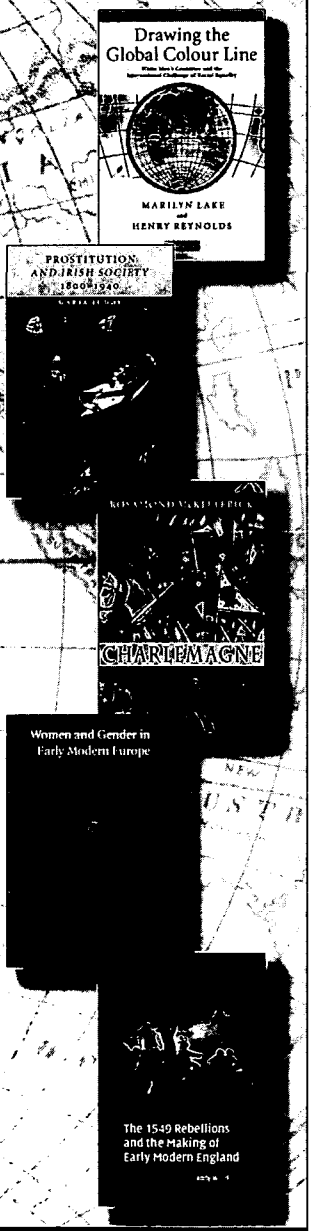
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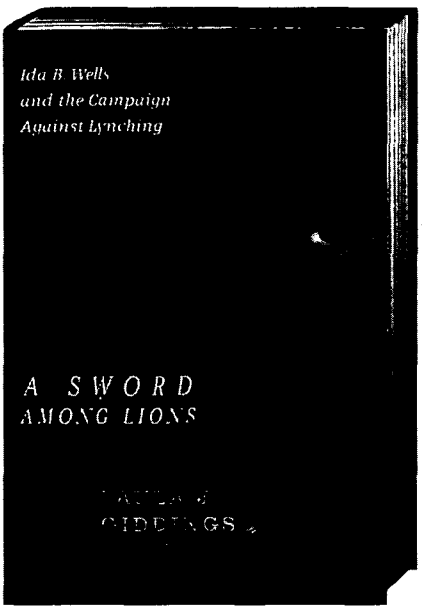


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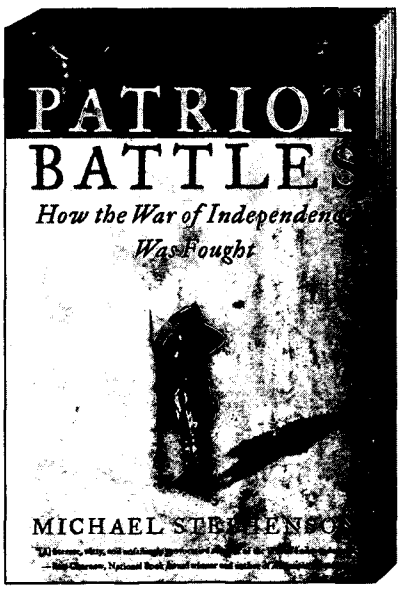
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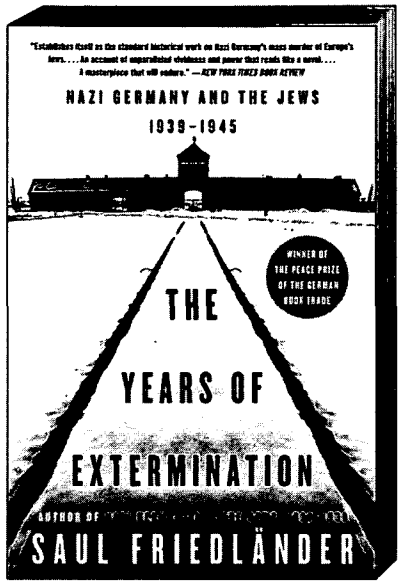


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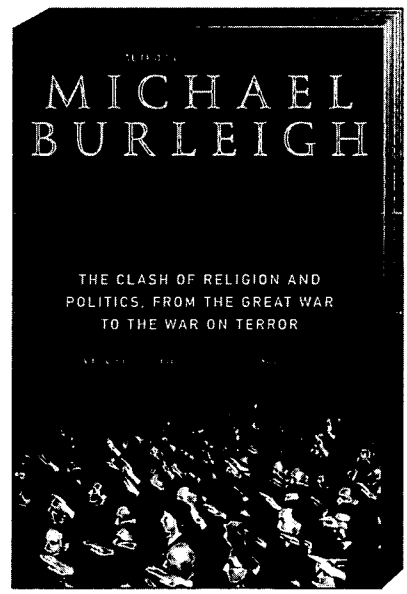
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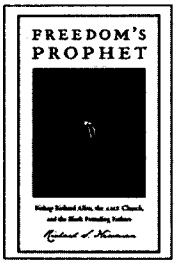
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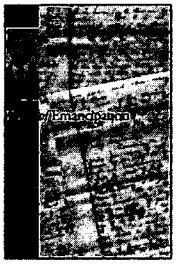
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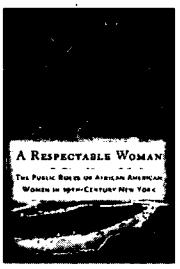
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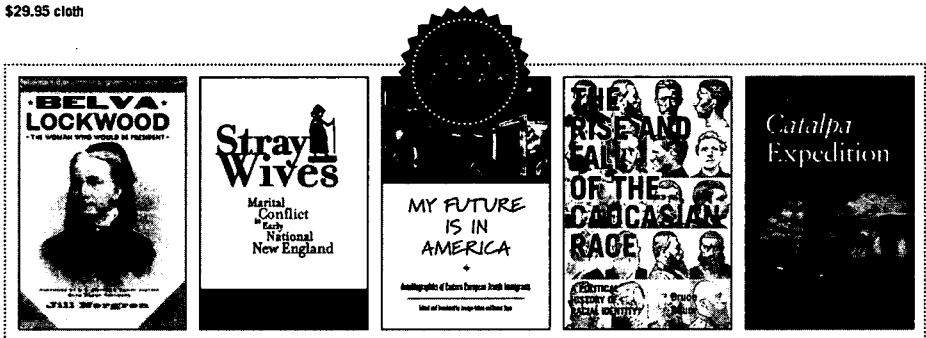
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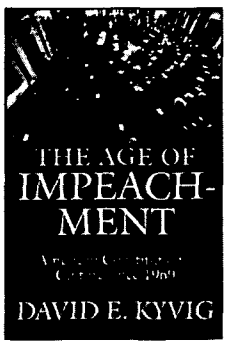
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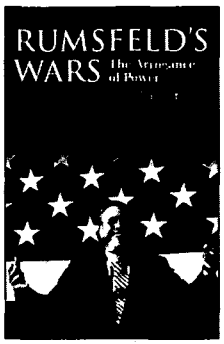
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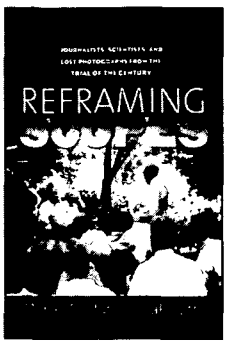
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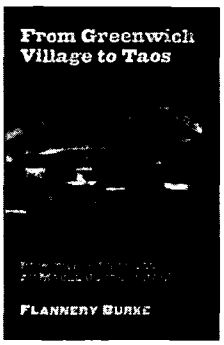
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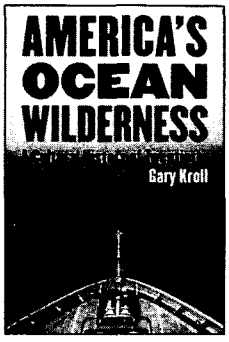
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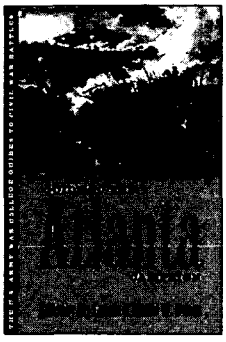
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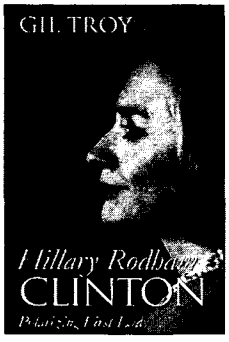
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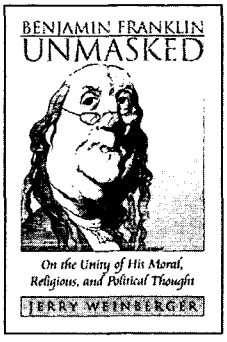
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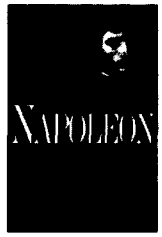
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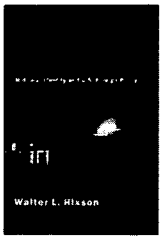
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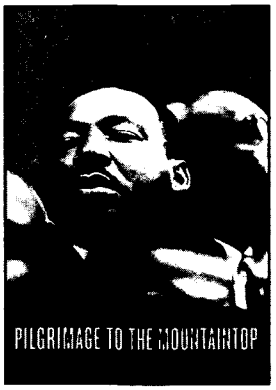
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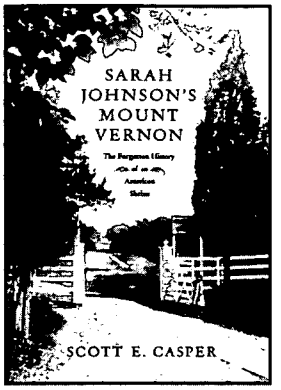
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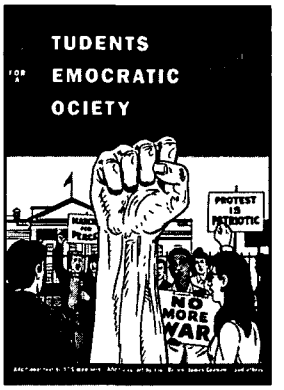
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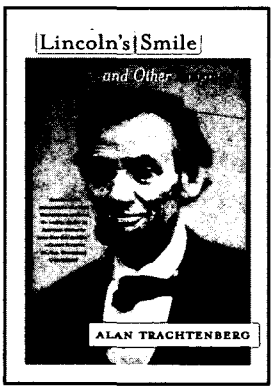
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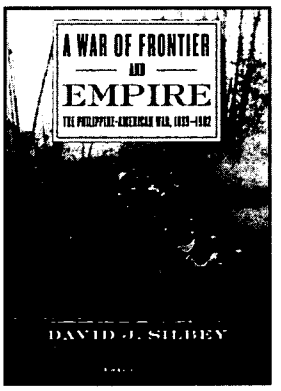
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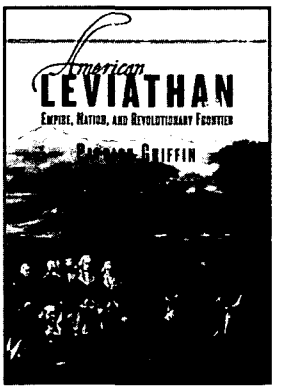
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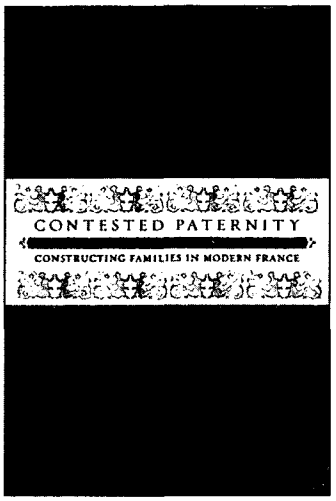
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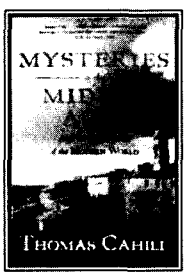
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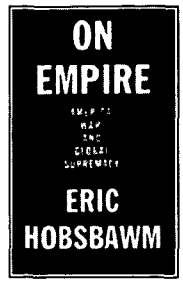
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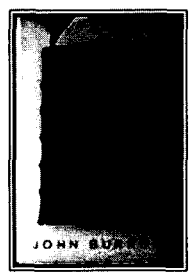
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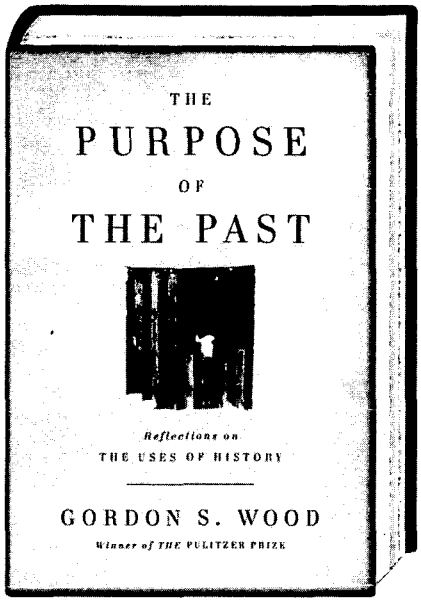
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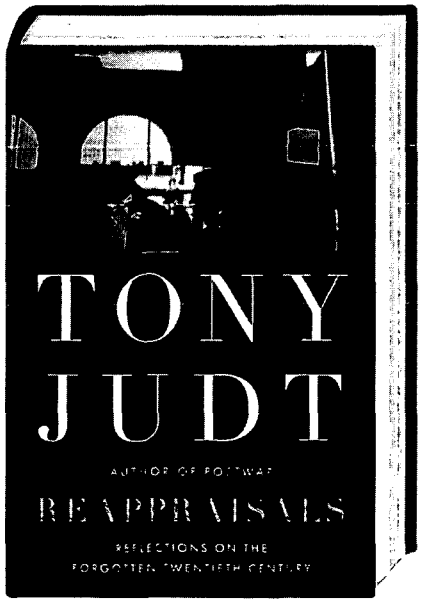
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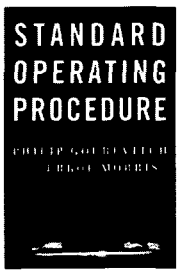


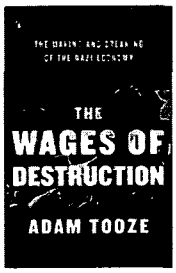
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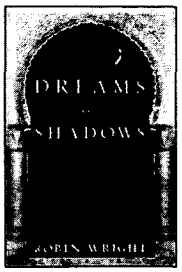
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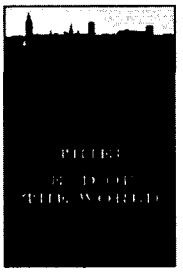
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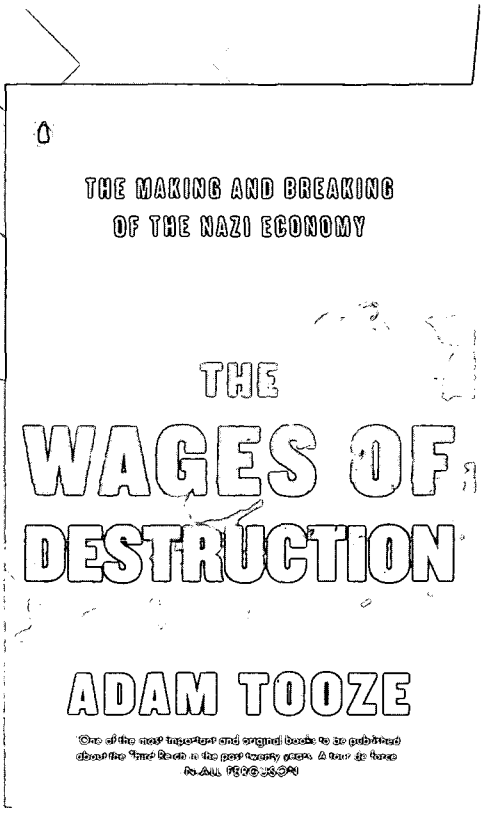
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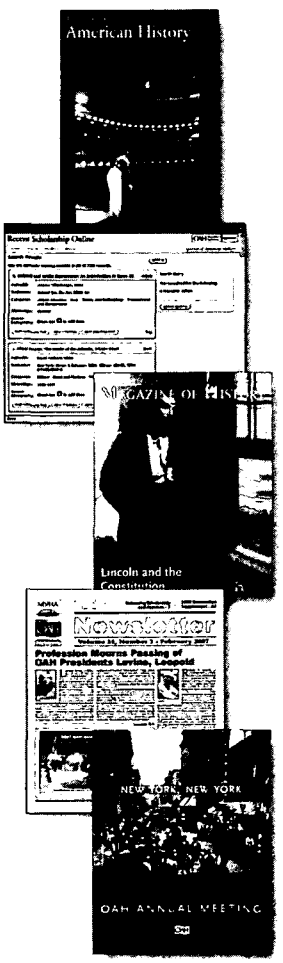
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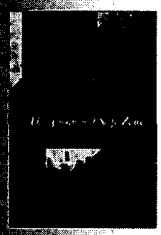
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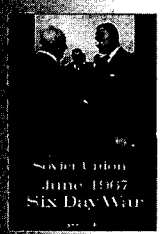
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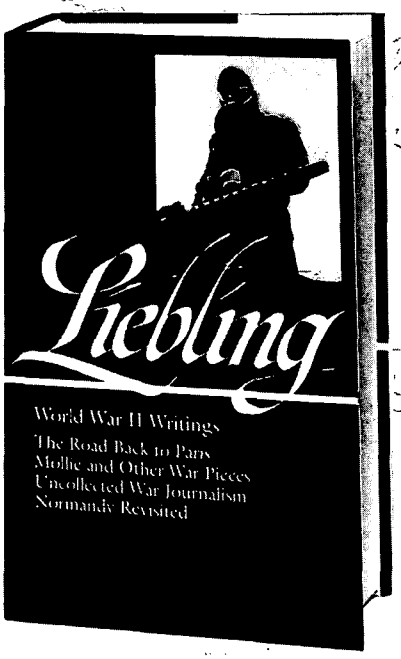
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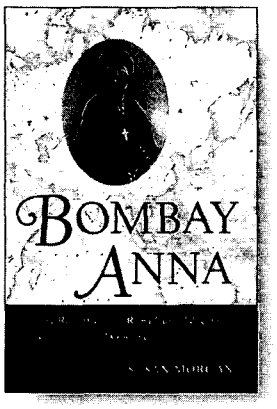
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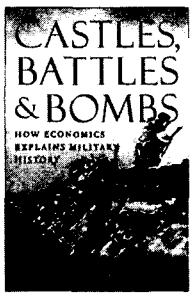
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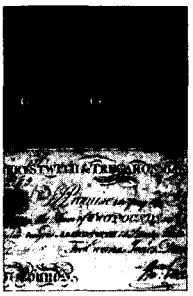
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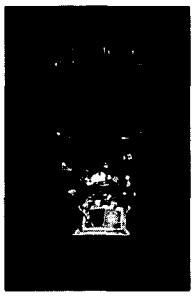
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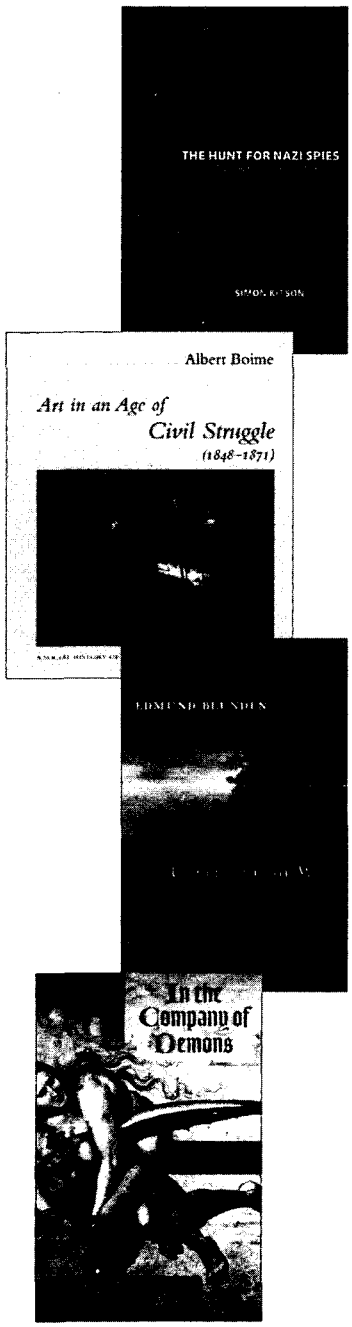
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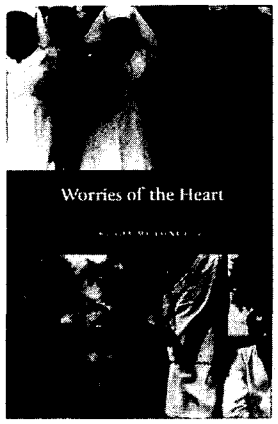
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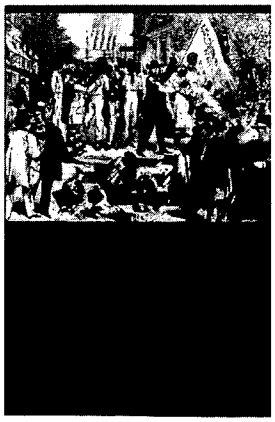
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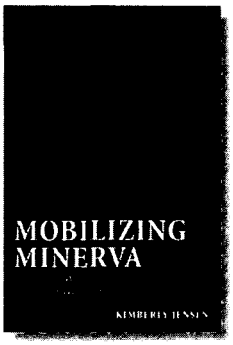
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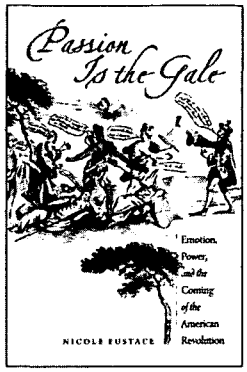
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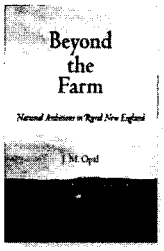
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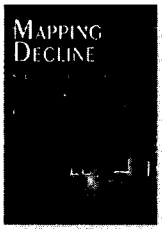
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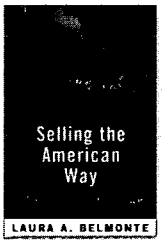
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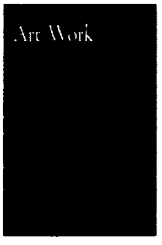
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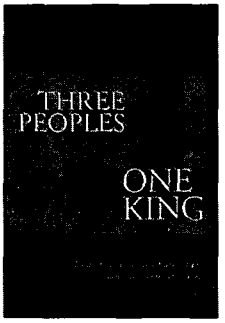
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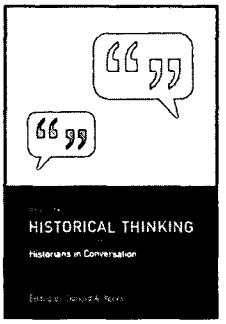
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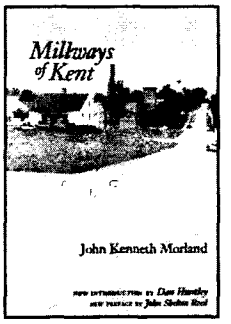
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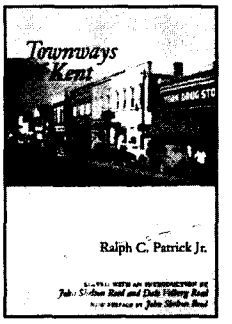
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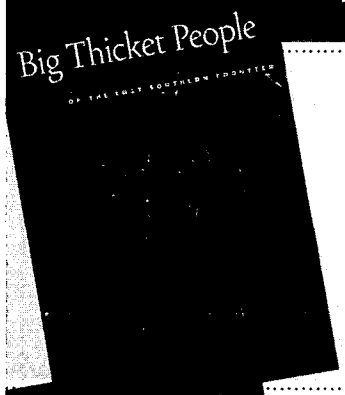




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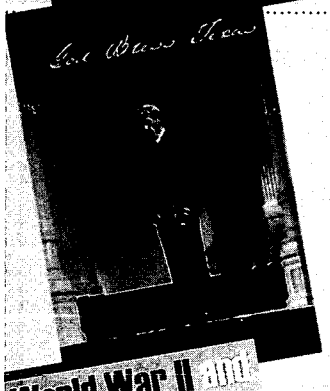
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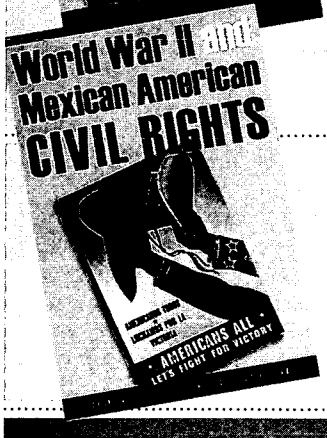


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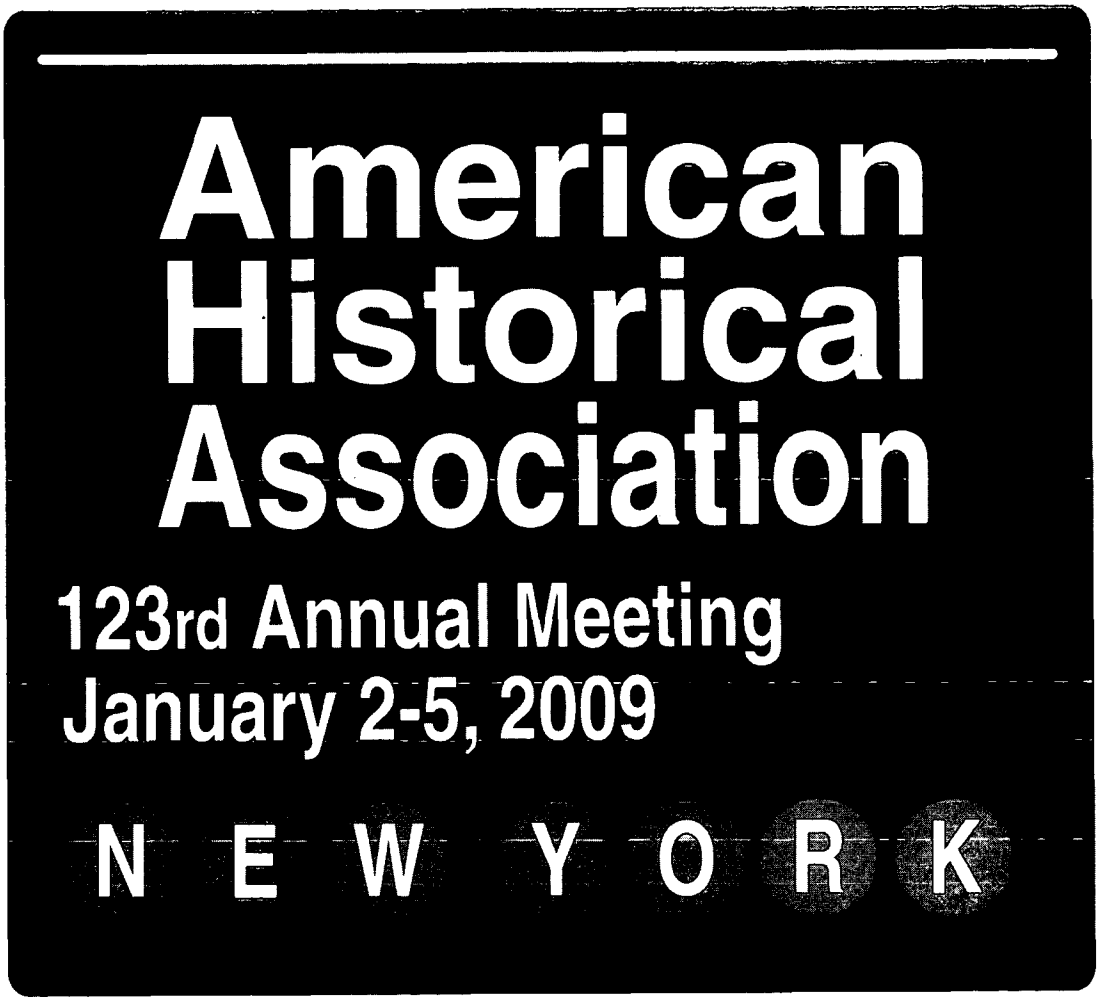
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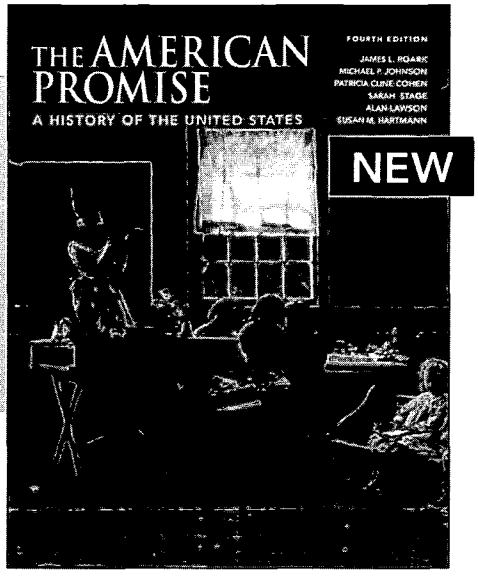
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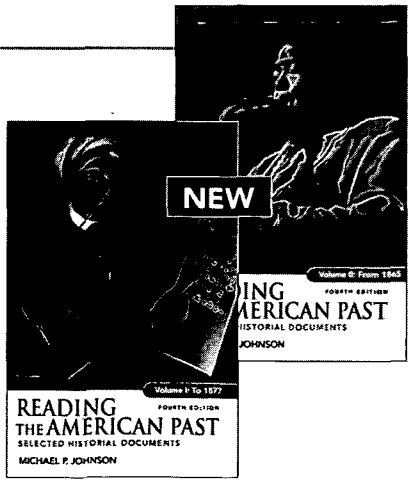


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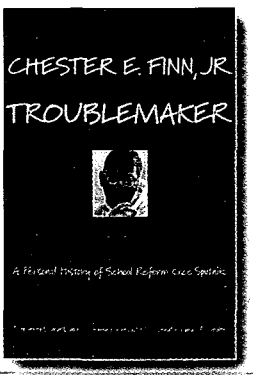
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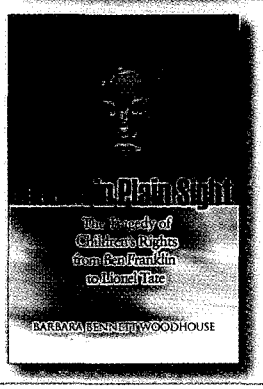
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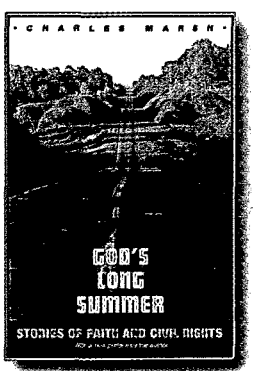
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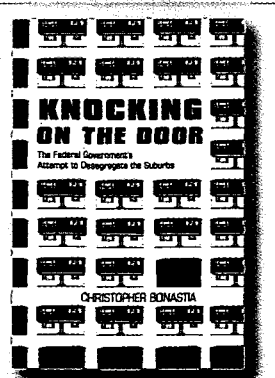
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